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SELECTION AND TRAINING OF STUDENTS FOR INDUSTRIAL RESEARCH[1]

By DR. ALBERT W. HULL

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IN selecting college graduates for industrial research, certain qualities are sought. Not all these qualities are the product of college training, but many of them might be. The purpose of this discussion is to inquire whether colleges could contribute more to this total training.

The qualities which are needed for success in industrial research may be classed in four groups. They are, in order of importance, character, aptitude for research, attitude toward work and knowledge.

CHARACTER

Character is the most important qualification. It is rated first in the hiring and retaining of personnel. I shall not try to define character; the many virtues which it comprises include self-discipline, courage and tolerance, all of which are needed in industrial research. It also includes honesty and generosity. Honesty is more than the negative virtue of not telling lies. Positive honesty is the quality that enables you to say of a man, "I always know just where to find him." It is the basis of true friendship and teamwork, and hence is an essential requirement in a cooperating group. The honesty of research workers, however, must be even greater than this. They not only must not deceive others but must not deceive themselves, for the greatest scientific sin is wishful interpretation of data. It is more common than is generally realized, and is so serious that it can reduce a research worker's value to the vanishing point. The man who is more anxious to prove that he is right

[1] Vice-presidential address before the Section for Physics, American Association for the Advancement of Science, Cleveland, 1944.

than to find the truth will nearly always arrive at a conclusion in accordance with his wishes. His observational values will be shaded, without intentional deceit, to agree with preconceived ideas, and he may even see things that are not there. A deeply grooved habit of scientific honesty, of facing the facts impartially, is more important than knowledge to a research worker.

Generosity, an old-fashioned virtue, is also necessary in the modern research laboratory. It is essential for cooperation, without which a laboratory to-day is primitive. Cooperation is a two-way flow; one can not expect to receive it without giving it freely and generously. It is important that this spirit prevail throughout the group, for one or two ungenerous men can poison the mutual confidence in a laboratory. Jealousy and greed and suspicion are weeds that grow easily, while cooperation is a delicate flower. Sometimes it is possible to isolate a single individual of exceptional qualifications and treat him as a prima donna, but only a genius is of much value as a prima donna. As a rule, no laboratory can afford to hire men who lack the generous spirit of cooperation.

If this analysis is correct, and character is the most important qualification of research workers, then it should be a prime objective of any educational system. Is it, in fact, the prime objective of our colleges today? I think all colleges would answer "Yes" to this question, but would have to admit that only a very small fraction of educational effort is devoted to it, because of lack of an accepted method of character training. Admittedly it is the most difficult as well as the most important of educational tasks. But when have universities shrunk from a task because it is difficult?

I am making this plea in the belief that what is needed is emphasis. War has taught us that tasks which seem impossible can be accomplished if given sufficient priority. Although this problem is being studied fundamentally by psychologists, and a better understanding of it may be hoped for eventually, we could, in the meantime, make greater use of what psychology has already taught. We know that character is neither inherited nor taught by precept, but is the product of self-discipline under the stimulus of environment. In earlier days the predominant environment was the home; to-day it is the school. For better or worse, we are "molded to manhood" under the influence of teachers and fellow students. Therefore, I plead for more consideration of character in the selection of faculty and students. Hard as it is to judge character, if we put it first as an entrance requirement, a much better job of selection could be done. To-day industrial laboratories put more emphasis on character in the selection of personnel than colleges do. It should be the other way around, since the college has the greater influence in molding men. Why not expend as much effort toward finding out whether applicants for admission have desirable character, as in scholarship tests? Some one may object that this is undemocratic, but it is not any more so than refusing entrance to the scholastically unfit. And education's method of raising the standard of scholarship by denying privilege to the unfit, is likely to succeed also in raising the standard of character by denying privilege to the ethically or morally unfit.

APTITUDE

Next to character, ability is the quality that is looked for in research men. For scientific work this means, of course, scientific ability or aptitude for research; its absence does not necessarily indicate lack of ability in other lines. The inborn aptitudes needed for research include imagination, analytical power and curiosity.

Imagination, the power to visualize situations and foresee possible consequences, is a prime requisite for pioneer research work. Without it a man can solve only the problems which are given him. He may be a useful man in a laboratory, but will seldom make discoveries. As a result, he is likely to be dissatisfied with his accomplishments in research, especially if he possesses other abilities, such, for example, as would make him a successful engineer or business man.

The power of analysis often is identified with ability. It is the power to size up a situation. By virtue of this gift the physicist is a problem-solver. Without it a man draws a wrong conclusion, or no conclusion at all, from every experiment. His advancement will be slow and he is likely to be dissatisfied, if he has skills that would enable him to excel in some other profession.

Curiosity is the most important research quality, in the opinion of one of the greatest pioneers of industrial research, Dr. W. R. Whitney. It is the quality that motivates research-for-the-fun-of-it. Nature is full of interesting problems to one who is endowed with this quality. If, in addition, such a one possesses the ability and character needed to make his research efforts bring satisfaction, and if, then, he is paid a salary for doing the thing he likes best to do, he is properly placed. I personally believe that every man has some useful ability or skill which gives him this kind of pleasure, and that each one could be properly placed.

The university's function in regard to ability is guidance. This is recognized by all colleges, and advice and placement are considered part of their responsibility. But it should be a much larger part, if the relationship between correct placement and happiness is as intimate as I have indicated. By placement I mean finding the right kind of activity rather

than getting a job. Few young men know what they want to do. They enter college with the question, "What have you?" The function of broad education is to show them what treasures of activity life holds; and to give them opportunity to try these different activities and find which ones gives them satisfaction. The time is short and the range of subjects is long. A correct placement can be found in four years only with the help of sympathetic and alert guidance from teachers, to see that undue time is not wasted in following false trails. More is involved than mere loss of time; for if a certain line of training has been followed to the degree stage, and then is found unsatisfactory, it is very difficult to make a change. The man feels impelled to follow a career that "makes use of his education."

I have purposely emphasized the importance of activity, rather than mere learning, as a goal in education. Much has been written about the treasures of knowledge—science, history, literature, philosophy—that a college should offer, in all of which I concur. But coincident with learning about these things, there should go the assessment of one's ability to contribute to them. The greatest satisfactions in life come from accomplishment, and such accomplishment is possible only in the line of one's greatest aptitude. Should not, then, the greatest emphasis in education be on learning *to do* by experiment—trying one's wings, so to speak? The broad college curriculum is ideally adapted to help each man determine what to do, provided sufficient emphasis is given to this objective. This is the first step toward correct placement. When men reach the professional-training stage, a second task devolves upon the university to advise and "screen" them, lest they choose wrongly because of ignorance, or from false standards, rating salary considerations above aptitude and congeniality. More careful screening at this level would be a great service to industry and could save many human tragedies.

ATTITUDES

While aptitude can only be recognized, attitudes can be taught, and, next to character, they constitute the most important product of education. This is no new doctrine; the greatest Teacher of all time taught that "As a man thinketh in his heart, so is he." Industry to-day is more interested in what a man thinks in his heart, that is, in his attitudes toward his work and his subject and his fellow men, than in what he knows. If he loves his work, so that days are too short, and has a passionate desire to know, which drives him to read and study, he will soon outstrip the most erudite pedant. This is especially true in the field of research. University training at best is too brief to provide more than a small fraction of the knowledge that is needed to make a good research man. The rest must be acquired during his career by continuous study. The opportunities are ample: books, fellow workers, up-to-date magazines and frequently study classes, sometimes sponsored by management but often organized and conducted by fellow workers. In emphasizing this self-education, I do not mean to underestimate the value of university training. The university has the task of imparting these attitudes which make self-education possible, and grooving them deeply so that they will endure. But it should not be forgotten that the *attitude* is the real goal of the training.

With this analysis every teacher and administrator will unquestionably agree. But most will also agree that attainment of this objective falls far short of the goal. The criticism does not apply to graduate schools, which on the whole, are doing an excellent job of imparting correct attitudes. Their success may be attributed partly to small classes, allowing personal contact with teachers; but mainly, I think, to the influence of fellow graduate students, who, having caught these attitudes from good teachers in the first instance, transmit them from class to class by infection.

It is this factor of student influence which also accounts for the relatively poor attitude toward work and study in most undergraduate schools. The "spirit" of a college is a living thing, which persists, transmitted from class to class, through decades and even centuries. Its persistence is one of the great factors in education, for the influence of fellow students is a more potent educational force than books or teachers. It is, on the whole, a healthy influence; but it is not often a scholarly influence. I believe that this attitude, caught from fellow students, spells the principal handicap in research work of the B.S. graduate compared to the man who has done graduate work. It is not so much the additional knowledge obtained in advanced graduate study as the changed attitude toward study that enables the one to advance without limit, while the other often reaches an early ceiling. Could this be changed? Could the *desire for knowledge* be attained at the undergraduate level?

There are other attitudes, toward work and play and friendship and tolerance, which the research worker needs, that also are products of group influence. Character, too, is molded by this influence. How can this all-powerful group spirit be controlled and molded? I shall suggest only one method, the method that works in industrial laboratories, namely, leaven. The beginning must be small—a qualified teacher with two students, then four, then eight, expanding only as fast as new members can be infected with the spirit of the group. Such a group might start within the walls of an established college, and as it grew could eventually become strong enough to leaven the whole college. It is a slow process, but the stakes are high. It might be worth trying.

KNOWLEDGE

Last and least of these four qualifications comes knowledge. But it is last only because of the high valuation of the other three, not because it is unimportant. The obtaining of new knowledge is the chief aim of research, and the accumulated knowledge of the past is its working capital. Moreover, the pursuit of knowledge furnishes the training ground on which the qualities of character and attitude are forged.

Science faculties to-day are giving a great deal of thought to the question: "What kind of knowledge is required for industrial research?" I shall attempt to answer this question.

From the standpoint of the laboratory with a long-range research program, the answer is: First and most important, fundamentals—as broad a range as possible. The reason for this is that the research man must be prepared to tackle more than one kind of problem. Research horizons expand, giving birth to new projects and making those of yesterday obsolete. In addition to this broad training, a man must master some one subject and become an expert in it, as part of his training, but *it matters little what that subject is.* For example, men trained in physical chemistry have made excellent physicists, and have become expert in such fields as high frequency electronics within a few months. Similarly, specialization in nuclear physics is a satisfactory training for industrial physics, provided it is combined with broad fundamental training.

For short-range research projects, the requirements are different. Here a man is hired to solve a particular problem or work on a particular type of development, with less emphasis on long-range usefulness. The requirements in this case are *training and expert knowledge in this particular field.* Broad fundamental training still is desired, however. The difference, therefore, is essentially only in the subject chosen for specialization; in the case of preparation for short-range research, the subject should be one for which there is a current demand.

SUMMARY

To summarize, I have tried to suggest that knowledge, valuable as it is, is not considered the most important qualification for industrial research. Character, aptitude and attitudes are more important. Should they not be rated so in the college educational program? Aptitude, though it can not be trained, can and should be screened by the college more than is done to-day, to avoid the lifelong disappointments of misplacement. Character and attitudes actually are molded by college influences, for better or worse, and therefore are products of college life and are the responsibilities of the college, just as much as scholarship. The fact that the molding influence is the student body, more than the faculty, makes the problem different from that of scholarship, and a different method must be found for dealing with it. Could a method be found if sought with sufficient effort? To-day we give it low priority, devoting only a small fraction of college effort to it. Why?

A STIMULUS-EXPECTANCY NEED-CATHEXIS PSYCHOLOGY[1]

By Professor EDWARD CHACE TOLMAN
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AT the time I sent in the peculiar title of this paper obviously I was suffering from mental obfuscation. However, in the intervening four or five months my mind has had time, I hope, to clear. In fact, I now seem actually to have discovered what the paper is to be about and to have invented a quite normal title for it, to wit: "The Contribution of Rats to Human Psychology."

In other words, what I really want to talk about is the simple, though somewhat hackneyed, subject of the contribution of rat experiments to the understanding of human behavior. It would seem that the ultimate goal of all psychologists (even of rat psychologists) is the explanation of the behavior of human beings. It appears further, however, that those of you, among us, who have concentrated primarily on human beings have become increasingly aware (as the psychologists of thirty or even twenty years ago were not) that human behavior takes place only in social contexts. You human-oriented psychologists have begun reading (and perhaps even *inventing*) bits of anthropology and sociology. As a result, you have finally become convinced that men are not born, like Athena, full-grown and all armored but, rather, as naked babes who begin acquiring their armor at their mothers' breasts, in the alarms and excursions of toilet training and in the give and take of sibling rivalries.

Now this has produced a real revolution in all our thinking—even in that of us rat psychologists. But, unfortunately, it seems also to have led to some tendency (or perhaps I am merely over-sensitive) on the part of you human psychologists now to look

[1] Address of the vice-president of Section I, American Association for the Advancement of Science, Cleveland, Ohio, September 12, 1944.

down, or rather up, your noses at us poor animal psychologists whom you seem to descry as still left hanging uncomfortably out on the limb—that is, on the good old philogenetic limb. And we animal psychologists (being very suggestible) actually begin to feel quite hang-dog about it all ourselves and we too begin trying to look snootily down (or up) our own noses at our own selves hanging by our own tails out on that same good old limb. But this I, for one, am finding a very uncomfortable position. Hence, the main purpose of this paper is to try to justify that limb to myself and to try to get myself right side up on it as rapidly, and with as much circumspection, as possible.

To return, now, to the new insight that human behavior is always behavior in a social, or culture, context, what have we animal psychologists left to do? The primates (that is, the chimpanzees and monkeys among us) have not been too hard hit. They have rapidly shifted their attention from problems of learning and insight to such problems as social dominance and submission; cooperation in work-activities; a pair of chimpanzees mutually picking off one another's fleas; the sexual cycle of the female ape and how the different stages of her genital swelling affect the food-generosity or the food-stinginess of her accompanying male; chimpanzees raised by human parents, and human babies raised by chimpanzee parents. Oh, no, I forgot. This last has not yet been tried. But it undoubtedly will be (although the folklore seems to prefer the she-wolf as the mother-image) by intention, or by default, if the world continues killing off human mothers at its present rate.

So much, then, for the primate psychologists; but what are we poor rodents to do? What can we contribute to the problems of human behavior, always enmeshed (as the latter is now so clearly seen to be) in the situations set by specific cultures and by specific social groupings? Our only comeback would seem to be that, whereas rats must be admitted to have very little social life and absolutely no culture, there are certain basic laws and principles which can still be studied more conveniently and with just as much validity in rats as in men.

But, first, let us consider further the nature of the new insights as to human behavior. Basically, these are merely the realization that every adequate description and every quantified rating of any aspect of human behavior must always involve and refer to a particular cultural milieu. For example, psychologists no longer conceive of I.Q.'s *per se* and abstracted from given cultural set-ups. Rather, they now realize that any given intelligence rating is always derived from performance with respect to the particular goals of a particular culture—goals with respect to which the individual in question has been reared and relative to which he now has to perform. Similarly, psychologists no longer think (or so I hope) of motivation *per se* but always of motivation with respect to those particular types of means-end lay-out provided by a particular society. Or, to take a final example, psychologists no longer consider emotional stability or its inverse emotional instability as something purely biological but rather as an entity which has final meaning only when defined by the norms and values of a going social group. For a man is emotionally stable or unstable not in a vacuum but by reference to the specific values which his culture prescribes. Thus, to have cataleptic-like trances was (we are told by Ruth Benedict) highly *de rigeur* on the part of women who would become shamans among the Shasta Indians in California. By having such seizures they then become counsellors of great power and importance. To quote: "It is clear that, far from regarding cataleptic seizures as blots upon the family escutcheon and as evidences of dreaded disease, cultural approval had zeized upon them and made them the pathway to authority over one's fellows. They were the outstanding characteristic of the most respected social type, the type which functioned with most honor and reward in the community."[2] But though cataleptic trances were thus highly approved among the Shasta Indians, obviously they would be rated as signs of emotional instability to-day in Cambridge, Mass. (or would they?).

But does this line of reasoning mean that we are now left with nothing but relativity? Do the words intelligence, motivation and emotional instability, taken apart from specific culture contexts, have no meaning? No, I do not intend to go as far as that. I would admit that there do still remain certain generalized and useful formal meanings for these terms. For, even though the actual material content and the accompanying quantified ratings of intelligence, motivation and emotional stability will not necessarily carry over from one culture to another—for example, from California to Boston, to the Trobriand Islands or to Cleveland—it will still be true that these terms are useful and necessary as generalized frames of reference. They are frames whereby we can compare behaviors of different cultures and the behaviors of different individuals in any one culture. Thus, as such formal frames of reference, what we mean by intelligence is probability of success in reaching goals; by motivation, probability of persistence in striving towards goals; and by emotional stability, probable tendencies not to exhibit unacceptable divagations in the pursuance of such goals. Intelligence is tendency to succeed, motivation is tendency to persist, and emotional stability is tendency not to exhibit unacceptable divagations.

[2] Ruth Benedict, "Patterns of Culture," Boston: Houghton Mifflin Company, 1934, p. 267.

On the other hand, it still remains true that we can not meaningfully rate the actual intelligence, the actual motivation or the actual stability of an individual raised in one culture, say, a Trobriand Islander, if set down in another culture, say, in New York (and having to react to a New Yorker's goals) or, similarly, those of a New Yorker set down in the Trobriand Islands (and having to react to the Trobriand Islander's goals). And there is still the further question as to in how far a New Yorker's intelligence, motivation and emotional stability, as rated in reaching one New York goal, will really carry over to, and are the same as, his intelligence, motivation and emotional stability in reaching other New York goals.

Let us consider this latter question further and let us begin with intelligence. Our first answer may be drawn from the dictates of common sense. Common sense undoubtedly would point out that some New Yorkers seem to be generally bright (successful) *re* all things New Yorkian, whereas others seem to be generally dumb (unsuccessful) *re* most things New Yorkian. This common-sense observation tends to lead, then further, to the notion of a single entity to be called "intelligence" or at any rate New York intelligence. And Spearman, as we all know, entertained this notion and sought to give it statistical validity in London and ended up with *g*.[3] However, as has already been argued, it is quite obvious that this London (or New York) *g* would not carry over, as such, to the Trobriand Islands. The man of high general intelligence in New York (or London) might well prove astonishingly dumb on the Trobriand Islands and, *vice versa,* the man generally dumb in New York (or London)—witness the Admirable Crichton—might prove surprisingly bright (that is, successful) on the Trobriand Islands. And so we return again to our original question. Is New York intelligence or London intelligence really unitary in spite of Spearman?

As a next step in the argument let us now shift our attention, from New York or London to Chicago. For in Chicago, as we all know, much brilliant statistical analysis has gone into showing that, at least along the shores of Lake Michigan, there are some seven, or is it nine, major subvarieties of intelligence. That is, Thurstone[4] and his students have demonstrated that Chicago intelligence seems to have at least the following major, and mutually independent, components: intelligence in words, in numbers, in spatial relations, in quickly perceiving visual or verbal meanings, in remembering relatively rote material, in inducing a general principle from presented particular data and in deducing particulars from presented general principles. That is, it has been found in Chicago that there are few interconnections between successes in reaching goals lying in the seven different directions of words, numbers, space, perceptually presented particulars, remembered particulars, induced relations and deduced particulars. Thus, for a man to be good (or poor) verbally does not mean any necessary concomitant tendency for him to be good (or poor) numerically, or spatially, or the rest. But, if he is good in reaching one goal in primarily verbal terms, he will also have a tendency to be good in reaching other goals which also lie primarily in the verbal direction (provided, of course, all the problems continue to be couched in good Chicagoese.).

Is this, then, our conclusion? Are there seven or perhaps nine basic kinds of intelligence capable of being universally generalized? At first sight, it might seem so. For it takes no great stretch of the imagination to suppose that what Thurstone found for Chicago would also hold for New York, London, for Cleveland or even (though I hate to admit it) for San Francisco. But consider again the Trobriand Islands. If Trobriand Islanders were raised in Chicago (and especially if they went to the University of Chicago) obviously there would be found the same outcome with them. But would we find these same seven "vectors of the mind" in the Trobriand Islands themselves? One doubts it. If a factor analysis were made of abilities on those islands, one might well find not seven but three or ten or fifteen dimensions of intelligence among which there might well prove to be some, to us, quite funny ones—such, say, as an ability to influence others by sorcery, which would correspond to no basic factor to be discovered in Chicago, Cleveland, London or San Francisco, though such a factor or ability might, perhaps, be found in Los Angeles.

My belief is, then, that that which Thurstone and his students have found are not seven biologically laid down aspects of intelligence but rather certain major directions of success prescribed by our Western culture-complex. We learn to perceive, to use words, to use numbers, to deal with certain types of spatial relations, to memorize nonsense materials (such as telephone numbers), to induce and to deduce. And each of these major learnings, whatever it may be based upon in the way of innate abilities, gets developed by the arrangements and accidents of our Western bringing up, more or less independently of each of the others. I do not pretend that such a hypothesis has really been proven, or that I have grounded it in actual data. I present it, however, as an *a priori* possibility and one which is, at least, worthy of examination. Moreover, it also makes sense when we return now, at long last, to rats.

For, it has been found that in all the intelligence

[3] C. E. Spearman, "The Abilities of Man," London: Macmillan and Company, 1927.

[4] L. L. Thurstone, "Primary Mental Abilities," Chicago: The University of Chicago Press, 1938.

problems which have been tried with rats there are extraordinarily small correlations between tendencies to succeed in any two different problems. One maze tends to correlate but little with another and mazes do not correlate at all with discrimination-boxes, or with puzzle-boxes, and the rest.[5] It appears, in short, that where culture does not operate (as in rats, thank God, it does not) intelligences (*i.e.*, tendencies to succeed) turn out to be very specific and almost unrelated to one another. This appears both in intercorrelation studies and from the further tests which, I understand, have been made on the Tryon and the Heron bright and dull strains.[6, 7] In short, I shall take very great biological specificity as my basic assumption. This notion I originally learned from Tryon.[8] I now believe with him (unless he has changed his mind in the meantime) that the biological and hereditary bases of intelligence are multitudinous, relatively narrow and very specific. Assortative mating, as it occurs in human societies, plus the coercive effects of our educational systems, have, however, the tendency to weld these multitudinous possibilities into a finite number (Thurstone's seven) culturally defined directions. The advantages (or disadvantages) of our systems of mating and of education tend to produce some human individuals who are good in most of the seven directions and others who are poor in most of them. But they also produce individuals who are good in some of the seven though poor in others. But again, I wish to contend that the particular seven categories, that we find, are not so many genetically segregating units but the product of a given culture. For once again, I would assert that the Trobriand Islander living on his islands might well be found by Thurstone to have not seven but say, fifteen or perhaps only three "vectors of the mind." And few, if any, of these fifteen or three might be found to coincide with the seven found in Chicago.

So much for intelligence. Consider now the second of our three sample variables—motivation. No good factor-analyses have yet, so far as I know, been made of human motivation in this country, although Spearman did claim to find in London a single generalized motivation factor, *w*.[9] However, though we do not have good correlation matrices for motivation, in human beings, we do have one for rats. E. E. Anderson[10] measured the motivation of 51 male albino rats in a variety of exploratory-driven, hunger-driven, thirst-driven and sex-driven problems. What did he find? First, there were no correlations from one drive to another. Secondly, in cases of the hunger and the thirst drives, he also found little or no correlations between the different measures of each of these drives by itself. That is, the rat who performed well as compared with the others, when all were under the influence of hunger, in one getting-to-food test did not necessarily perform well as compared with the others, when all were again under the influence of hunger, in another getting-to-food test. And a similar lack of intercorrelations was found between different measures of the thirst drive. Thirdly, in the cases of the exploratory and the sex drives, however, he did find evidences of something which carried over from one test to another. The rats who were highly exploratory in one apparatus (which encouraged exploration) did show some tendency also to be the ones who were highly exploratory in other apparatuses, which likewise encouraged exploration. Similarly, he found intercorrelations between different measures of the sex drive. The rats who copulated most frequently when a receptive female was present also tended to be the ones who dug most rapidly through sand to get to such a female; and the like. In short, some motivations, such as hunger and thirst, when measured (as in rats) outside the grouping effects of a culture appeared very specific and contingent upon the features of the particular situation. Others, however, such as exploration and sex appeared more general and less tied to specific situations.

When now we turn to human beings, although the corresponding studies have not been made with them, it seems probable that much evidence of generalized and more culturally determined drives would also be found. Thus, for example, some men in our culture would appear to be highly motivated in the whole area of scholarly pursuits but poorly motivated in those of sports or business. Others would appear to find great motivation in the general area of being good husbands and fathers while others would show but little of that generalized drive and would exhibit, rather, strong drives for writing poetry, painting pictures or for an exaggerated night life. But here, again, though we might well find such an appearance of "vectors of motivation," it would seem obvious that as in the case of the "vectors of intelligence," they would be mostly the products of a particular civilization and not of human biology. There are certain major goals which our culture sets up. And, growing up in this culture, some of us, due no doubt in part to our special inheritances but probably much more as a result of the accidents of early training and experience, tend to pick up some of these goals and others of us tend to pick up others of them. If, to use the vernacular, an individual has been "raised right," he may acquire most of them, but, if he has not been "raised right,"

[5] C. L. Vaughn, *Comp. Psychol. Monogr.*, 14: No. 3, 1937.

[6] R. C. Tryon, *39th Yearbook. National Society for the Study of Education*, 1940, Part I, 111–119.

[7] W. T. Heron, *Jour. Comp. Psychol.*, 19: 77–89, 1935.

[8] R. C. Tryon, *Jour. Comp. Psychol.*, 30: 283–336, 1940.

[9] *Op. cit.*

[10] E. E. Anderson, *Comp. Psychol. Monogr.*, 14, No. 6, 1938.

he may acquire but few—or only those of lesser repute. And an individual raised in a totally different culture might well acquire almost none of our major motivations. Again the doctrine, I am contending for, is that of an as yet perhaps unknown set of basic biological drives upon which given cultures then build their own smaller or greater number of culturally defined and specified motivational directions.

In fact, it would seem that Freudian psychology and its offshoots and derivatives considered (not as therapy) but as explanatory principles are no more and no less than some stimulating hypotheses as to how a given culture, working through the early family set-up and the early training procedures characteristic of the family and of the larger culture in which the family is immersed, may operate to emphasize in given individuals certain of the major motivational directions of the given society and in other individuals, others of those major directions.

Finally, turn to the last of our three variables which I have suggested—merely by way of example—that of emotional stability. This is, undoubtedly, the as yet least clearly conceived of the three. I defined it above as the tendency of the individual not to exhibit irrelevant and unacceptable divagations in the pursuance of a given goal. Let us turn now once again to rats. Let us consider Hall's pioneer studies.[11] You will remember that his rats were deposited one by one in an open field and records were kept of how much they moved about, how much they defecated and how much they urinated. And he found that the individuals who defecated and urinated most also tended to be the ones who did the least moving about. Such animals tended to freeze in one spot. Furthermore, he and his students have also found that two different strains can be bred—one a strain high in defecation, urination and immobility and one a strain low in these propensities.

The question which arises next is then, how general (or how specific) is this complex of responses? I confess to not being too clear on the point. But, for the sake of argument and to bring the discussion in line with what I have contended for the other two variables, I shall again adopt a similar position. I shall assert that these tendencies, which Hall and his students have bred for, constitute a relatively specific biological entity. And I shall contend further that these tendencies to defecate, urinate and freeze into immobility, taken by themselves, can not, as such, be evaluated as either emotionally stable or unstable. They are biological substrata upon which a culture may or may not build If the rats themselves had a culture, they might either disdain or promote to the status of seers and prophets the individuals who showed such propensities. In the first case, such propensities would be said to be symptoms of instability; in the second case, symptoms of genius and of greatness. I suspect, however, that rats, if they had a culture, would, like men, be more apt to find such propensities bad. They might well feel about them as we feel about bed-wetting beyond a certain age. Though in a strictly neutral sense bed-wetting may, for all I know, be a nice outlet for certain deep-lying motivational conflicts.

In short, my argument is that emotional stability, like intelligence and motivation, is, in the last analysis, an evaluative and cultural concept. It depends upon the rules of the given culture which behaviors are to be defined as unacceptable divagations and which are to be designated, rather, as incidental, colorful (and perhaps desirable) accompaniments of the carrying out of prescribed goals. Holding to this point of view, all I now wish to emphasize about such researches as those of Hall and his students is that they are important contributions to the problem of the uniqueness and the inheritability of certain funny types of propensity which cultures may then either utilize (or condemn) and weld into either what they call stability or into what they call instability.

This concludes my purely descriptive evaluation of the three basic psychological concepts—intelligence, motivation and emotional stability. I have emphasized that I believe all three to be *von Grund aus* cultural concepts. They can not be given specific contents divorced from the particular cultures in which they operate. And the significance of the work with rats was that it proved in each case that the inherited bases may be relatively specific and may have no simple one-to-one relationships with the finally molded culturally defined variable.

Finally, however, there is one further point which I, as a rat psychologist, must raise. For if, as I have been arguing, there are no unitary intelligence, motivation or stability functions necessarily common to all men in all cultures and still less common to men (who operate in cultures) and to rats (who operate outside of cultures), what, it may be asked, is the significance of most of our rat studies other than those on heredity? What about the thousands of studies on learning and on motivation and the smaller but pioneer number of studies on conflicts in rats? My answer would be to assert that, although rats have no culture, still the formal laws about the causation and development of intelligence, motivation and instability are universal in character and can be examined in rats just as well as, and far more conveniently than, in men.

And this brings me back once again to my original abortive title—"A Stimulus-Expectancy Need-Cathexis Psychology." And now I must add still a third neologism; namely, Conflict-Instability, so the com-

[11] C. S. Hall, *Psychol. Bull.*, 38: 909–943, 1941.

plete title would read: "A Stimulus-Expectancy, Need-Cathexis, Conflict-Instability Psychology. In other words, there are, I believe, three basic types of causal determiner (to wit, stimuli, needs and conflicts) which may be thought to be the respective primary causes of our three variables. And the equations involving these determiners and other factors such as numbers of repetitions, primacy, recency and the like, which connect these determiners to the final three variables, can be better studied in rats than in men.

The basic laws of intelligence concern the fact that successive re-presentations of arrays of environmental stimuli arouse in an organism "sign-gestalt-expectations" (as I originally called them) or what Hilgard and Marquis[12] have called, more simply, "expectancies." I am grateful for and shall accept their shorter term (although it is possible that I may use it in ways they didn't intend). Thus an intelligence functioning (that is, a success functioning in the reaching of a goal) is, as I see it, an expectancy on the part of the organism, aroused by that part of the stimulus lay-out which is immediately presented, to the effect that such and such performances or behaviors (if carried out) would be successful in reaching such and such a goal. These expectancies fundamentally are merely sets in the nervous system aroused by environmental stimuli. In the case of human beings such neurally-based expectancies are (as we know) often accompanied by consciousness; but they need not be. And, in any case, their definition does not involve the question as to whether or not they are conscious. It is pointer-reading behaviors which operationally define them. To sum up, the total causal factors underlying such expectancies are, as I see it: (1) the presented environmental stimuli; (2) the hereditary determinants of ability, whatever they finally turn out to be; and (3) the laws of learning (*i.e.*, sign-gestalt, or expectancy, formations). And the operation of all these basic factors and laws can be as well studied in rats as in men—even though (as I have insisted, probably by this time *ad nauseam*) the particular expectancies which get built up in men are determined and guided by particular cultural set-ups and even though, also, the amounts and kinds of repeated presentations (and the span of environmental entities, offered in any one presentation) are likewise, in the case of men, also culturally determined. The basic shape and equations of the learning curve can still be determined by the study of rats, and far more conveniently than by the study of men.

Turn, next, to motivation. The basic problems of motivation I have tried to epitomize by the hyphenation—need-cathexis. That is, motivations are derived basically from the arousal of needs plus the added fact that (through heredity and/or through training) certain types of goal-objects get cathected by a given need. Such cathected goal-objects, when reached, relieve the need. It now appears, further, that the basic laws concerning the arousal of needs—especially in the case of the simple viscerogenic needs—may likewise be successfully studied in rats. And the basic laws of cathexis whereby particular goal-objects get cathected by particular needs can also be studied in rats. Some important beginnings have, in fact, already been made by P. T. Young in his studies of food-preferences.[13] But the further (and humanly more interesting) problem of how, on the basis of the simple viscerogenic needs, the more complicated psychogenic ones (to use Murray's dichotomy)[14] get built up, probably can not (I must admit) be studied in rats—although it probably can be, and in large part is being, studied in chimpanzees. But problems such as the list of major psychogenic needs and of the concrete types of goal which get cathected by them has obviously to be investigated separately in each culture. In other words, although possibly we can study in chimpanzees the basic laws of what I have elsewhere called "drive-conversions,"[15] that is, the conversion of the libido of the viscerogenic needs into the libido of various psychogenic needs, we can not, I fear, approach the study of the actual goal-aims of the psychogenic needs of men in various actual cultures except by studies within those cultures.

Turn now, finally, to our third sample variable—emotional stability. Here we have as yet very few general principles (whether in rats or in men). But, as I have already indicated, I believe the appropriate neologism would be conflict-instability. That is, I am supposing that it is conflicts between two or more needs which are the basic causal determiner of those kinds of behavior which a given culture will declare to be symptoms of emotional instability. Those particular irrelevances and divagations (such, for example, as bed-wetting, nail-biting, stammering, flushing, cataleptic trances, visions and hallucinations) which a given culture may either disdain or capitalize upon, result primarily, when two needs conflict with one another. Along side of some major overt need some second covert need is at work and interferes with the attaining of the major goal. The individual is having to handle two (or it may be more) needs at once and it is this which causes the "funny" behaviors. Hall's rats would seem, for example, to

[12] E. R. Hilgard and D. G. Marquis, "Conditioning and Learning." New York: D. Appleton-Century Company, 1940.

[13] P. T. Young, *Jour. Comp. Psychol.*, 1932, 14, 297–319; *Jour. Comp. Psychol.*, 15: 149–165, 1933.

[14] H. A. Murray, "Explorations in Personality." New York: Oxford University Press, 1938.

[15] E. C. Tolman, "Drives toward War." New York: D. Appleton-Century Company, 1942.

have been interrupted in their exploring by their coincident fear. But as to the basic laws which made some of the individual rats more susceptible to such interference than others we as yet know practically nothing. We do not know whether Hall's more stable rats (in calling them more stable we are, of course, evaluating them as if they were human beings living in our culture) were so because they had inherited little fear or because they had inherited better "inner walls" for keeping their different need-compartments separated (to borrow Lewin's figure).[16] And, if we did know this for rats, we certainly do not know it for men. Is the emotionally stable man in our culture one who has no conflicting needs or is he rather one whose tough compartmentalized make-up keeps his competing needs from interfering? Or is he perhaps, quite oppositely, one whose needs do interfere but in such a way that the culture considers him a leader or a genius? We do not know. In any case, however, it is clear that, while it will be desirable to work out more of the basic principles of need-conflict with rats, it also has to be confessed that special studies with men in their own actual cultural set-ups likewise will be necessary. For, again let me emphasize that the "funny" behaviors which are termed instability in one culture may be called genius or at least a peculiar delightfulness and richness of coloring in another.

But enough. What, by way of summary, can we now say as to the contributions of us rodent psychologists to human behavior? What is it that we rat runners still have to contribute to the understanding of the deeds and the misdeeds, the absurdities and the tragedies of our friend, and our enemy—*homo sapiens?* The answer is that, whereas man's successes, persistences and socially unacceptable divagations—that is, his intelligences, his motivations and his instabilities—are all ultimately shaped and materialized by specific cultures, it is still true that most of the formal underlying laws of intelligence, motivation and instability can still be studied in rats as well as, and more easily than, in men.

And, as a final peroration, let it be noted that rats live in cages; they do not go on binges the night before one has planned an experiment; they do not kill each other off in wars; they do not invent engines of destruction, and, if they did, they would not be so dumb about controlling such engines; they do not go in for either class conflicts or race conflicts; they avoid politics, economics and papers on psychology. They are marvelous, pure and delightful. And, as soon as I possibly can, I am going to climb back again out on that good old philogenetic limb and sit there, this time right side up and unashamed, wiggling my whiskers at all the dumb, yet at the same time far too complicated, specimens of *homo sapiens,* whom I shall see strutting and fighting and messing things up, down there on the ground below me.

[16] K. Lewin, "A Dynamic Theory of Personality." New York: McGraw-Hill Book Company, 1935.

OBITUARY

DEATHS OF RUSSIAN BOTANISTS

A FEW names have to be added to the long list of Russian botanists—the victims of total war (see SCIENCE, 100: 43–44, 1944).

The most irreparable of these losses is the passing on April 19, 1942, of Professor Aleksandr Aleksandrovich Elenkin (1873–1942), one of the most prominent authorities on cryptogams. He was born on September 4, 1873, at Warsaw and educated in the university of the same city. After serving one year as an assistant in botany at the University of Warsaw, he was appointed a conservator of the St. Petersburg Botanical Garden and since then was always associated with that institution, which later was incorporated into the Academy of Sciences as its institute of botany.

Elenkin was active in all fields of cryptogamic botany. His first works were on the lichenology of Russia. He made several exploring and collecting trips to Finland (1898–1909), Caucasus and Crimea (1899), eastern Siberia and Mongolia (1902) and Central Russia (1903, 1907, 1910). On the basis of these collections he published his classical work, "Flora of Lichens of Central Russia," in four parts (1906–11). His studies of mosses are represented by another important work—"Mosses of Central Russia" (1909). In 1906 he was appointed director of the phytopathological station of the Department of Agriculture, and this turned his attention to mycology and phytopathology. He edited the journal *Boliezni Rastenij* ("Morbi plantarum") and contributed many papers to it. In 1910 he was given the task of describing the algae of the Kamchatka expedition of F. N. Riabushinsky (1908–09) and four years later published "Die Süsswasseralgen Kamtschatka's" and "Die Meersalgen Kamtschatka's" (In "Expédition à Kamtchatka" 2: 1–448, 1914). This was the beginning of his thirty years' study of the algae of Russia; in this field he became an undisputed authority in the Soviet Union and culminated his life's work by a masterpiece, "Monographia algarum cyanophycearum aquidulcium et terrestrium in finibus URSS inventarum" (1936–38), two volumes of which are published up to date and two others will be issued after the war. Besides this, he was the author of several papers on Darwinism and the philosophy of

botany. The full list of his published works includes 472 titles.

Georgij Karlovich Kreyer (1887–1942), who at the time of his death on January 11, 1942, was in charge of the Section of Medicinal Plants of the Institute of Plant Industry at Leningrad, was justly considered one of the best experts on medicinal plants of Russia. Born on November 26, 1887, at St. Petersburg, he was educated in the same city and first studied the meadow and swamp vegetation of St. Petersburg province. But since 1916 he devoted himself to the task of the cultivation of medicinal plants. He made a great contribution to the solving of the extremely difficult problem of growing the quinine tree on the Black Sea coast of Caucasus. He also organized the cultivation of insecticide plants and many medicinal plants such as valeriana, belladonna, opium poppy, aloe, etc., on various stations of the Institute of Plant Industry (VIR). His most important publications are: "Medicinal plants," two volumes of which are published up to date; "The cultivation of medicinal plants," with V. V. Pashkevich (1931, ed. 2–1934); "Valeriana officinalis in Europa und im Kaukasus" (1930) and monographic studies of Atropa, Scopolia, Valeriana, etc. He is the author of more than 60 published works.

Ivan Andreevich Ohl (1884–1943), who died on February 19, 1943, in Kazan, was for many years a bibliographer and librarian of the St. Petersburg Botanical Garden (now the Institute of Botany of the Academy of Sciences) and an authority on botanical bibliography. Prior to this (1909–21) he worked at the phytopathological station of the St. Petersburg Botanical Garden and published twelve papers on mycology and phytopathology. In the field of botanical bibliography he is mostly known for his eight classified bibliographies of Russian botanical works for the years 1930–37 published in the journal *Sovetskaia Botanika* and two bibliographies of algological works (1929, 1935) compiled by him in collaboration with A. A. Elenkin. His vast knowledge of botany, botanical literature and several languages, general erudition and an attractive personality made him an ideal librarian for a large scientific institution.

Viktor Konstantinovich Zazhurilo (1909–1943), killed in action on January 8, 1943, was a young phytopathologist of great promise. Born in 1909 at Tula and graduated in 1930 from the University of Voronezh, he started his work at the agricultural experimental station at Voronezh, becoming later a senior specialist of the phytopathological station of the same city. He specialized first on the diseases of beans, and then turned his attention to the virus diseases of the Gramineae. He is the author of 26 papers on mycology and phytopathology. He proved himself a brilliant experimentator and a master of laboratorial technique.

Vladimir C. Asmous

Arnold Arboretum,
Harvard University

DEATHS AND MEMORIALS

Dr. William Henry Howell, professor of physiology and director of the School of Hygiene and Public Health at the Johns Hopkins University from 1926 until his retirement in 1931, died on February 6 in his eighty-fifth year.

Dr. Irving S. Cutter, dean emeritus of the Medical College of Northwestern University, died on February 2 at the age of sixty-nine years.

Professor Samuel J. Record, dean of the School of Forestry of Yale University, a member of the faculty since 1910, died on February 3. He was sixty-three years old.

Dr. Wayland Morgan Chester, professor emeritus of biology of Colgate University, died on February 8 at the age of seventy-four years.

Mme. Jean Cotelle, a former associate of Mme. Curie, has died from the effects of handling large quantities of radioactive substance.

Josiah Willard Gibbs, from 1871 until his death in 1903 professor of mathematical physics at Yale University, has been nominated for 1945 to the Hall of Fame at New York University.

A memorial meeting commemorating the scientific and industrial achievements of the late Dr. Leo Hendrik Baekeland, inventor of the first modern plastic, was held at the Hotel Roosevelt, New York City, on February 9 by the American Section of the Society of Chemical Industry and the New York Section of the American Chemical Society. About three hundred and fifty chemists and engineers participated. The speakers included Dr. Wallace P. Cohoe, a former president of the Society of Chemical Industry, and George K. Scribner, president of the Boonton Molding Company, Boonton, N. J.

SCIENTIFIC EVENTS

BOOKLETS OF INFORMATION FOR LATIN-AMERICAN BIOLOGISTS

As a step toward the establishment of more intimate relationships between biologists of the two Americas, the Union of American Biological Societies has recently published booklets dealing with graduate instruction and research in the biological sciences in the United States. A booklet for Spanish readers bears

the title, "Organización de los Estudious Superiores de Biologia en los Estados Unidos" and a similar publication for Portuguese readers, "Altos Estudos e Pesquisas no Domínio das Ciências Biológicas nos Estados Unidos." The booklets are being distributed to biologists and biological centers throughout the Latin-American republics.

These publications have been prepared especially for the use of Latin-American biology professors and students who contemplate coming to the United States for advanced work. Among the subjects discussed are the organization of biological studies in the graduate school and in professional schools, such as those of medicine and agriculture, and the requirements for admission and for advanced degrees. Opportunities for study at various types of biological field stations are described and also the rôle which biological societies and publications play in the promotion of research. The booklets explain the methods for making application for scholarship and fellowship aid.

Organization of university work in the United States, both at the undergraduate and at the graduate level, differs in many primary respects from that which prevails in the various Latin-American republics. It is hoped, therefore, that the publication of booklets of information will serve, not alone to stimulate interest among qualified Latin-American students in the possibilities for biological study and research in the United States, but also as an aid to such students in preparing themselves as adequately as possible for advanced work. The booklets should be of use, also, to Latin-American professors who desire to come to this country for short sojourns, either at a university or at a biological station.

In the printing and distribution of the booklets, the Union of American Biological Societies has received the cooperation of Science Service. Copies of the booklets are now being mailed directly to biologists throughout Latin America. Any one in the United States desiring a copy of either the Spanish or the Portuguese booklet may secure one by writing to Dr. E. G. Butler, president of the Union of American Biological Societies, Princeton University, Princeton, N. J.

E. G. BUTLER

THE AMERICAN MUSEUM OF NATURAL HISTORY

AT the seventy-sixth annual meeting of the Board of Trustees of the American Museum of Natural History, held on January 29, officers of the board were elected as follows: F. Trubee Davison, *President;* A. Perry Osborn, *First Vice-president, Acting President,* during the absence of Colonel Davison, now in war service; Cleveland E. Dodge, *Second Vice-president;* E. Roland Harriman, *Treasurer,* and Clarence L. Hay, *Secretary.*

Mr. Osborn gave a report on the progress of plans for future rebuilding and modernization of the museum and the contributions made by friends of the museum during 1944. According to this report:

After two years' consideration the Management Board and the Administration have approved post-war building plans which will shortly be filed and which involve the complete rehabilitation of the fifty-year-old 77th Street buildings, with new heating, lighting and ventilation throughout as well as reconstruction of the present facade. The other main features of the plan are the erection of a substantial new three-story building from Ninth Avenue going east to the present auditorium, thus providing essential modern storage facilities, as well as new first- and second-floor exhibition space, tying in with the present Ninth Avenue dead ends; the conversion of the present Hall of Ocean Life into a large, modern auditorium with over twice the seating capacity of the present one, and the relocation and modernization of the scientific library.

During the next few years, the Administration will be extremely busy in perfecting plans for the change and very little or no new exhibition can be undertaken. This time lag I believe to be a great advantage, since it will enable a truly great scheme to be developed, bringing all the latest scientific knowledge to combine with the most effective exhibition techniques. We plan to dismantle the old North American Hall on the second floor and use this space for temporary exhibits, as well as to try out new exhibition techniques and secure public reaction in experimental displays.

The science of visual instruction is still young and museum plans for the modernization of at least twenty great halls, including such subjects as the ecology of nature, geology, soil, forestry and botany, paleontology, insects, fishes, reptiles, birds, mammals and man, present a challenge which only the most careful scientific research and the very best in exhibition methods can adequately meet.

THE WATSON SCIENTIFIC COMPUTING LABORATORY AT COLUMBIA UNIVERSITY

A COMPUTING laboratory has been established at Columbia University by the International Business Machines Corporation. It will be known as the "Watson Scientific Computing Laboratory at Columbia University." It plans to serve as a world center for the treatment of problems in the various fields of science, whose solution depends on the effective use of applied mathematics and mechanical calculations.

Dr. Wallace J. Eckert, formerly director of the Nautical Almanac at the U. S. Naval Observatory, who was recently appointed director of the department of pure science of the International Business Machines Corporation, is in charge of the laboratory.

Research and instructional resources will be made available to scientific workers, universities and re-

search organizations in this country and abroad, and special cooperative arrangements will be made with scholarly institutions. Columbia University and the laboratory will engage in a joint program of research and instruction, utilizing for this end the personnel and facilities of both institutions.

In addition to the regular members of the scientific staffs of the university and the corporation, special consultants include Dr. Harvey N. Davis, president of Stevens Institute; Dr. William P. Tolley, president of Syracuse University; Dr. William Mather Lewis, president of Lafayette College, and Dr. T. E. Brown, of the Harvard Business School.

Columbia University has assigned a building at 612 West 116th Street on Morningside Heights to the laboratory.

The work will start as soon as the necessary alterations in the building and the installation of equipment have been completed.

REORGANIZATION OF THE SHEFFIELD SCIENTIFIC SCHOOL OF YALE UNIVERSITY

By vote of the Corporation of Yale University, recently announced by President Charles Seymour, a reorganization of the Sheffield Scientific School will take effect on July 1.

Undergraduate courses in the sciences leading to the degree of Bachelor of Science, heretofore under the administration of the dean and faculty of the school, will be transferred to Yale College, thus consolidating under a single faculty all undergraduate education in the liberal arts and sciences.

As a result of this reorganization, the function of the Sheffield Scientific School in the future will be two-fold.

(1) The Board of Permanent Officers of the school will constitute the committees of the Graduate School on the graduate degrees in the sciences and mathematics and will recommend candidates for these degrees to the faculty of the Graduate School. Candidates for these degrees will be registered as students in the Sheffield Scientific School.

(2) The director and the Board of Permanent Officers of the school will constitute the Division of Science and Mathematics of the university and as such be responsible for maintaining and promoting, with the approval of the president and the corporation, programs of study and research in the natural and physical sciences and mathematics. This division will include the following departments of study: anatomy, astronomy, bacteriology, botany, chemistry, clinical medicine, forestry, geological sciences, mathematics, pharmacology, physics, physiological chemistry, physiology, psychology, public health, zoology, and such other departments or organizations for scientific study and research as may from time to time be appropriately included within the division.

It will be the function of the division to maintain and promote study and research in the several fields included within the division, and in performing this function the Board of Permanent Officers is authorized to appoint annually, upon nomination of the director, an Executive Committee in the Physical Sciences and Mathematics and an Executive Committee in the Biological Sciences.

It will be the duty of the director with the advice, assistance and approval of these executive committees to review all proposals for appointment and promotion in the departments of the division or when it appears to be necessary, to initiate such proposals, and to transmit to the schools concerned and the president its recommendations on such proposals. The director shall in like manner make recommendations to the appropriate schools designed to improve courses and subjects of instruction in the sciences and assist the departments in their plans for research.

Departmental budgets in this division will be reviewed by the dean or any school served by a particular department. The director and the deans concerned shall act as members of the committees making final budgetary recommendations to the president in the fields of study included in the division.

Dr. Edmund W. Sinnott, Sterling professor of botany and chairman of the department of botany, has been appointed director of the school.

SCIENTIFIC NOTES AND NEWS

The Cedergren Gold Medal of the Royal Institute of Technology of Sweden was presented on February 9 at a ceremony held in New York City to Dr. E. F. W. Alexanderson, consulting engineer of the General Electric Company. The award, which was established by the late Henrik Tore Cedergren, is given once in every five years. It was presented by Martin Kastengren, Swedish Consul General.

Rear Admiral Edward R. Stitt (retired), from 1920 to 1928 Surgeon General of the United States Navy, was presented on February 5 with the gold medal of the American Foundation for Tropical Medicine in recognition of "outstanding service" in that field. The award carried with it a gift of $500. The presentation was made at a dinner at the University Club, New York City. Colonel Richard P. Strong,

director of tropical medicine at the Army Medical School, Washington, and recipient of last year's annual award, presented the medal.

DR. EDWIN RICHARD GILLILAND, professor of chemical engineering at the Massachusetts Institute of Technology, now on duty with the Office of Scientific Research and Development, Washington, D. C., has been chosen the first recipient of the Leo Hendrik Baekeland Award of the North Jersey Section of the American Chemical Society. The award, consisting of $1,000 and a gold medal, goes biennially to an American chemist under the age of forty years. The award was made in recognition of outstanding achievement in the fields of heat transmission, diffusion, distillation and high pressure synthetic chemistry. Dr. Gilliland is known as an advocate of the maintenance of a postwar synthetic rubber industry in the United States as essential to the national interest. Formal presentation of the award will be made at a meeting in Newark, N. J., on May 14. The award was founded to commemorate the technical and industrial achievements of Leo Hendrik Baekeland and to encourage younger chemists to emulate his example.

THE John Jeffries Award of the Institute of Aeronautical Sciences for outstanding contributions to the advancement of aeronautics through medical research was presented on January 31 at a meeting of the institute at the Engineering Societies Building, New York City, to Air Marshal Sir Harold E. Wittingham, director general of the medical services of the Royal Air Force.

THE ten recipients of the medal of the Typhus Commission of the United States of America include Captain Thomas Jerrell Carter, Medical Corps, U.S.N., in charge of the Bureau of Medicine and Surgery of the U. S. Navy Department, and Major Charles M. Wheeler, Sanitary Corps, A.U.S., member of the staff of the International Health Division of the Rockefeller Foundation.

ON the occasion of the retirement of R. Kent Beattie, of the Division of Forestry of the Bureau of Plant Industry, his associates gave him a testimonial luncheon at the bureau cafeteria when he was presented with a brief case and a book of letters from his many friends.

DR. GILBERT GROSVENOR, president of the National Geographic Society, has been elected a fellow of the California Academy of Sciences.

DR. C. LANGDON WHITE, professor of geography, has been elected president of the National Council of Geography Teachers for 1945. He has served as first vice-president of the council since 1942.

OFFICERS of the executive committee of the Division of High-Polymer Physics of the American Physical Society for 1945 are: *Chairman,* W. F. Busse; *Vice-chairman,* S. D. Gehman; *Secretary,* W. J. Lyons, and *Treasurer,* L. A. Wood. Other members are H. Mark, newly elected to the committee, R. B. Barnes, a previous incumbent, and J. H. Dillon, the retiring chairman.

THE following officers of the Torrey Botanical Club, New York City, have been elected to serve in 1945: *President,* Dr. Fred J. Seaver, New York Botanical Garden; *1st Vice-president,* Dr. John A. Small, Rutgers University; *2nd Vice-president,* Dr. A. E. Hitchcock, Boyce Thompson Institute; *Corresponding Secretary,* Dr. Jennie L. S. Simpson, Hunter College; *Recording Secretary,* Dr. Frances Wynne; *Treasurer,* Dr. Edmund H. Fulling; *Editor,* Dr. Harold W. Rickett, all three of the New York Botanical Garden; *Bibliographer,* Mrs. Lazella Schwarten, Arnold Arboretum, Mass.; *Business Manager,* Miss Ann M. Hanson, Columbia University.

DR. G. RICHARD WENDT, of Wesleyan University, has been appointed chairman of the department of psychology of the University of Rochester. He succeeds Dr. Elmer A. Culler, who is retiring on account of ill health.

DR. DONALD V. JOSEPHSON, assistant professor of dairy husbandry at the Pennsylvania State College, has been appointed associate professor in the department of dairy technology of the Ohio State University.

DR. WALTER H. SEEGERS, research biochemist in the laboratories of Parke, Davis and Company, has been appointed associate professor of physiology at the College of Medicine of Wayne University.

AT the University of London, Dr. L. S. Penrose has been appointed to the Galton chair of eugenics tenable at University College. Since 1939 he has been attached to the Provincial Department of Health, Ontario, Canada, and in addition has been physician at the Ontario Hospital, lecturer in psychiatry at the University of Western Ontario and medical statistician for the Province. Dr. C. Rimington has been appointed from May 1 to the university chair of chemical pathology tenable at University College Hospital Medical School. Since 1937 he has been on the staff of the National Institute for Medical Research.

DR. AUGUSTUS B. WADSWORTH, since 1914 director of the Division of Laboratories and Research of the New York State Department of Health, Albany, N. Y., retired on February 1.

DR. C. W. VICKERY, senior statistician of the Office of Defense Transportation, has become consulting

engineer in Washington, D. C., specializing in industrial, actuarial and quality control engineering.

The Forest Preserve District of Cook County, Illinois, has recently established a Conservation Department under the direction of Robert Mann. Dr. David H. Thompson, formerly of the Illinois State Natural History Survey, is serving as zoologist.

Dr. Nolan D. C. Lewis, director of the Neurological Institute of the Columbia-Presbyterian Medical Center, has succeeded Dr. Smith Ely Jelliffe, who retired on January 1 as editor of *The Journal of Nervous and Mental Disease* and *The Psychoanalytic Review*.

Dr. Paul Hugh Emmett, since 1937 professor of chemical and gas engineering at the Johns Hopkins University, now on leave of absence for war work, consultant of the U. S. Department of Agriculture, has joined the research staff as senior fellow of the Mellon Institute, where he will plan and supervise long-range investigational projects on catalysis that are of importance in petroleum technology.

Dr. George W. Jeffers, professor of biology, has a year's leave of absence from the State Teachers College at Farmville to direct a survey of the fisheries resources of Chesapeake Bay. The study is being financed by the General Education Board of the Rockefeller Foundation and was established by the Chesapeake Bay Fisheries Commission, with Dean Ivey F. Lewis, of the University of Virginia, as director and made up of representatives of both Maryland and Virginia institutions, not directly connected with the fisheries industry but concerned about its future. The commissioners of fisheries of both states are also members.

Dr. Fred C. Bishopp, assistant chief in charge of research of the Bureau of Entomology and Plant Quarantine of the U. S. Department of Agriculture, Washington, D. C., will deliver the annual Hermann M. Biggs Memorial Lecture of the New York Academy of Medicine on April 5. He will speak on the "Medical and Public Health Importance of the Insecticide DDT."

Dr. William C. Rose, professor of biochemistry at the University of Illinois, delivered on January 18 the Elias Potter Lyon Lecture at the Medical School of the University of Minnesota. His subject was "The Amino Acid Requirements of Man."

The address of the retiring president of the Philosophical Society of Washington was given on January 6 by Dr. Harold Frederic Stimson, of the National Bureau of Standards. He spoke on "The Measurement of Some Thermal Properties of Water."

The fourth Annual Lecture Series at the Morley Chemical Laboratory of Western Reserve University will be held from February 23 to March 30 and from April 20 to May 25. The subject of the series is "Frontiers in Chemistry." The following lecturers will take part: Professor H. S. Taylor, Princeton University; Dr. Harold H. Strain, the Carnegie Institution; Dr. Robert J. Myers, The Resinous Products and Chemical Company; Professor Hans Mueller, the Massachusetts Institute of Technology; Professor Ernst A. Hauser, the Massachusetts Institute of Technology; Professor J. W. McBain, Stanford University; Professor I. M. Kolthoff, the University of Minnesota; Professor John H. Yoe, the University of Virginia; Dr. Otto Beeck, the Shell Development Company; Dr. James Hillier, the Radio Corporation of America; Dr. Frederick D. Rossini, the National Bureau of Standards, and Dr. John A. Hipple, Westinghouse Electric and Manufacturing Company.

A request to the War Committee on Conventions for permission to hold the meeting of the American Mathematical Society in New York City on February 24 was submitted by the president and secretary of the society. Permission has been denied and the meeting has been cancelled.

The Council of the American Association of Pathologists and Bacteriologists has voted unanimously to cancel the scientific meetings for the year 1945.

The Executive Committee of the Eastern Division of the American Philosophical Association has decided that the annual meeting must again be cancelled. This action was taken in response to the following telegraphic message from the director of defense transportation. "Your attention is directed to Justice Byrnes' request for the cancellation of group meetings, conventions and trade shows to be held after February 1. While your meeting is not as large as many others, and while we are aware of your past cooperation, we respectfully suggest that prompt cancellation would be a fine and patriotic action and would contribute to the war effort."

A Conference on Gene Action in Micro-Organisms was held under the auspices of the Missouri Botanical Garden, St. Louis, on February 2 and 3. The attendance at the conference was about forty, fifteen research laboratories of the United States being represented. The program will be published within a few months in the *Annals* of the Missouri Botanical Garden.

The Southern California Association of Physics Teachers was organized as a group on December 16, following the program of the American Physical Society at Pasadena. The initial members numbered about forty. The following officers were elected:

President, Dr. David L. Soltau, University of Redlands; *Vice-president,* Dr. Roland R. Tileston, Pomona College; *Secretary-Treasurer,* Dr. Laurence E. Dodd, University of California at Los Angeles. The group applied for a charter as a regional chapter of the American Association of Physics Teachers. This application was granted by the national Executive Committee at its meeting in New York on January 18. The new group is the tenth member of the Regional Chapters. The first program meeting of the new chapter is planned for March 24 at the University of Southern California, with several invited papers on the general subject "Physics Teaching and the Post-War World," to be followed by a group of contributed papers.

SCIENTIFIC workers in the United States are collaborating with their Latin-American colleagues in making available files of journals for new and active institutions where the lack of adequate library facilities has greatly impeded research. Two libraries have already received a gift of journals. The Committee on Inter-American Scientific Publication, of which Dr. Harlow Shapley, director of the Harvard Observatory, is director, is now collecting journals for a number of other Latin-American institutions. Those having unused files of journals that they are willing to contribute are invited to communicate with the Comité Interamericano de Publicación Científica, Harvard College Observatory, Cambridge 38, Mass.

THE Helen Putnam Fellowship of Radcliffe College for advanced research in the field of genetics or of mental health, which carries a stipend of $1,900, will be awarded annually, beginning with October 1, 1945, for an eleven-month period, with the possibility of a renewal. Appointments will be limited to mature women scholars who have gained their doctorate or possess similar qualifications and who have research in progress. All normal laboratory facilities will be provided to the holder of the fellowship. Applications for the award must be submitted to Radcliffe College not later than April 15. The first holder of the fellowship will be announced about the middle of May.

THE *Times,* London, reports that to express its gratitude to Holland, where numerous Zurich physicians were educated, especially at Leyden, the Swiss University of Zurich has decided to contribute to the revival of the University of Leyden. It contemplates the reconstitution of various departments and of the library, and will perhaps send professors there.

DISCUSSION

STAFFING SCIENCE DEPARTMENTS AFTER THE WAR

ALTHOUGH the December setbacks in Europe have for the time being caused attention to veer sharply from postwar problems, attention can never safely be completely withdrawn from the reconstruction period. It is unwise not to view present actions in the light of their postwar effect, as well as in the light of the needs of the present.

There is every indication that the war has raised the prestige of science and technology tremendously. Smaller businesses are anxiously casting about for ways of gaining access to the fruits of research. Strong political support may accelerate the tendency to channel some scientific effort in this direction. Larger businesses are planning to expand their research activities. In the cases of some laboratories plans call for doubling the size and scope of their activities. The Government, and particularly the military, are planning vastly increased research activity. Much of this planning is even now under way. The beginning of sharp competition for the best brains of the war research laboratories is clearly discernible.

Internal quickening of the demand for high-grade personnel is, however, only a part of the picture. The lesson of this war, that the possession of a sound and creative technology is the sine qua non of military and economic security, has been learned by every country in the world through the bitterest of lessons, in many cases. There is plenty of evidence that foreign countries will uniformly pursue a policy of intensive technological renaissance. This new movement will concern America in two ways. There will be aggressive attempts to hire American engineers and scientists and there will be a flow of students to American universities. For example, India has a very ambitious program and has established an office in Washington to recruit technological personnel and to facilitate the enrolment of students in American universities. A number of other nations have already made tentative overtures. The extent of these movements can only be guessed at present, although the Division of Cultural Cooperation of the State Department is alert and observant and may be expected to gauge the trend as it takes shape.

In the face of these enhanced demands for technically trained personnel, the American policy during the war has not fostered an increased supply. In fact, the training of technical personnel has been largely stopped. The effect of this is discussed in an article in the January number of the *Scientific Monthly.* It is sufficient to state here again that the

American stock pile of scientific personnel has now reached a peak and for a time will progressively be reduced by the usual forces of attrition, until adequate training programs resume.

The danger here is that the colleges and universities will not be able to compete for personnel, either in point of salary or in point of time. Where industry or foreign countries will be able to offer immediate inducements, the American universities and colleges will often have to wait for an assured budget and an adequate student body before committing themselves to paying faculty salaries. Already an appreciable number of former faculty members, who have been in the services and have resigned their commissions, have found attractive industrial positions waiting for them. Although they may have preferred teaching positions, suitable ones were not open, since colleges and universities are not now in a position to add to their staffs.

Unfortunately, no simple remedy for the problem can be suggested. Recognizing the danger, Dr. James B. Conant recently spoke out urging industrial laboratories not to "kill the goose that lays the golden egg." However, individual laboratories are in no position to be forbearing. If they do not hire the bright young men, their competitors will. And foreign countries will, of course, be guided only by their needs and resources. Subsidization of colleges and universities to permit them to build staffs carries with it certain unfortunate controls which are undesirable. Other more direct manpower controls are even more abhorrent. The most satisfactory of all solutions would be an aggressive policy of selection and training of scientifically apt youth. In this way a supply might be reached which would obviate the necessity of rigid controls in this field which otherwise seem inevitable.

It is to be hoped that the crucial importance of resuming scientific training at all levels will soon be recognized and that the colleges and universities will be able to make proper adjustments to present altered conditions, so that they may successfully build their staffs to meet the mounting postwar training load. It is not too much to state that the future welfare of the nation is closely involved, in view of the central role of the sciences in military and economic spheres.

M. H. Trytten

Office of Scientific Personnel,
National Research Council

"PSYCHOLOGICAL DIFFERENCES AS AMONG RACES"?

Professor Garrett's recent communication[1] on the correctness of Professor Montagu's statement[2] regarding the question of the demonstration of valid differences in psychological characteristics among races poses problems of a psychological and semantical nature.

Professor Garrett states that "investigations of race differences in this country have regularly and consistently found differences as between Negro and white" and that, though such differences may be interpreted differently, the "fact of their existence can not be denied." Such a statement, when read in conjunction with Professor Garrett's general comments, seems to imply that there does exist a firm factual basis for the contention that there are racial differences in psychological characteristics. The available body of fact does not seem to support this contention.

While it is a fact that studies in which various features of cultural difference were not controlled have frequently purported to demonstrate the existence of differences in various test performances as dependent upon the variable of "race," the admissibility of such evidence as worthy of serious consideration is, indeed, questionable. The demonstration of differences in test scores between white and Negro Americans does not at all necessarily constitute evidence in support of innate racial differences. Such differences in test scores could only be interpreted as representing inherent differences in racial capacity if all the other variables which affect test score were carefully controlled. The most obvious of such variables are socio-economic status, education, general cultural surroundings, urban or rural domicile and rapport between subjects and examiner, all of which are independently capable of yielding significant differences in performance. Thus, a simple demonstration of differences in score as among races on a psychological test does not mean that racial differences have been demonstrated, but only that different mean scores have been obtained for different ethnic groups. This is quite a different matter from demonstrating "psychological differences as among races." The most that can be concluded from such studies is that two test populations, which differ among other things in skin pigmentation, tend to make different mean test scores.

Perhaps the most interesting and candid rejection of the "evidence" for a racial basis of the differences found to exist in test performances in the so-called comparative racial studies has come from the late Professor C. C. Brigham, whose own study[3] formed a corner-stone of the racial differences in intelligence theory. In a review[4] article appearing seven years after the publication of his extensive comparative study Brigham concluded, on the basis of the then available evidence, that:

[1] Science, n.s., 101: 16–17, 1945.
[2] Science, n.s., 100: 383–384, 1944.

[3] C. C. Brigham, "A Study of American Intelligence," Princeton, N. J., 1923.
[4] C. C. Brigham, *Psychol. Rev.*, 37: pp 158–165, 1930.

This review has summarized some of the more recent test findings which show that comparative studies of various national and racial groups may not be made with existing tests, and which show, in particular, that one of the most pretentious of these comparative racial studies—the writer's own—was without foundation (p. 165).

In his survey[5] of the information available on the question of racial differences, Professor Klineberg clearly indicates the artificial character of the "facts" of racial difference, and states that "the conclusion came first, and the 'facts' were found to justify it" (p. 344).

In the recent studies of race differences in which careful attempts have been made to control some of the non-racial variables affecting performance, as well as in the best conducted of the earlier studies, the purported differences in performance as among races have not been demonstrated with any degree of clarity. In summary, it can be said that, so far as the psychological evidence is concerned, on the one hand there has been no clear demonstration of facts which are unequivocably interpretable as evidence for the existence of racial differences, while on the other hand there does exist a growing body of evidence which indicates that as cultural factors are controlled the obtained differences in psychological characteristics as among races tend to diminish to the point of insignificance.

HERBERT G. BIRCH

REGENERATION OF ADULT MAMMALIAN SKELETAL MUSCLE IN VITRO[1]

IN the course of an *in vitro* study of normal synovium from the patellar region of an adult rat, we have noted the appearance, growth and spontaneous contraction of striated muscle fibers from the muscle component of the explants.

On the 14th and 15th days of culture, muscle fibers were noted in two cultures; on the 16th day, the fibers began to contract spontaneously; and on the 18th day, the fibers still contracting, cross striations were observed. A portion of the explant also contracted rhythmically and spontaneously. These cultures were kept for six days more, during which time they were washed and fed about three times. The fibers increased in number and length. During this period they retained the capacity to contract spontaneously and rhythmically, at different rates and intervals. One fiber with no connection to the explant was also seen contracting. The contraction was most vigorous and involved the greatest number of fibers immediately after washing and feeding. Often there was a period of relatively rapid rhythmic contraction followed by a period of inactivity, after which contraction was resumed.

The outgrowing muscle fibers were ribbon-like structures with nuclei occurring singly, doubly, or in threes, fours or fives along their length. Longitudinal striae could be seen in the ribbons. Cross striae were elusive in nature, appearing, disappearing and reappearing. The majority of fibers showed no cross striae throughout their length even when contracting.

The muscle fibers are similar in structure and behavior to those of rat embryos (intercostal region) grown in this laboratory. In both cases the Maximow method was used, *i.e.*, a flying coverslip upon which the cells grew undisturbed except when being washed and fed. The only differences observed were that the embryonic muscle fibers were broader and had a greater number of nuclei. Longitudinal striae could be seen; cross striae were elusive, as in the case of the adult fibers. The muscle fibers in the embryo began to contract spontaneously at between four and eight days. One culture retained its capacity to contract for over a month.

In summary: Striated muscle from adult rat was seen to regenerate and to contract spontaneously *in vitro;* in appearance and behavior these fibers were similar to embryonic rat striated muscle fibers cultivated by the same method.

Since innervation of any sort is eliminated, this capacity of skeletal muscle to fibrillate *in vitro* offers a method for further analysis of contractility.

IRENE A. POGOGEFF
MARGARET R. MURRAY

ORTHOGRAPHY OF SCIENTIFIC NAMES

THE recent article by Dr. Harold Kirby[1] is a fine summary of the complex problem of transcription and orthography in scientific nomenclature. It omits one point which has had considerable importance in the past and continues to plague systematists. Transliteration has a strong nationalistic flavor. Many authors have been concerned to produce a version of the Greek or Latin term which looked French, Italian or German, etc. The real problem is: Are we rendering letters or sounds? Actually we compromise the matter by doing sometimes one and sometimes the other. This difference is illustrated by the official Russian and Library of Congress systems of transliterating Russian. The first conveys the spelling in Roman without helping a Western European to pronounce the word, while the latter gives the pronunciation but no idea of the Cyrillic spelling.

To look at some of Dr. Kirby's examples: *Agchylostoma* gives the Greek spelling but not the sound,

[5] Otto Klineberg, "Race Differences," Harper and Brothers, N. Y., 1935.

[1] From the Department of Surgery of the Presbyterian Hospital, and the Laboratory of Surgical Pathology, Columbia University, New York, N. Y.

[1] SCIENCE, 100: 425–427, 1944.

Chlamidomonas and others represent the usual and incorrect sound of upsilon. *Y* and *ph* are letters or combinations that are unfamiliar or unpronounceable to Italians. I doubt if most Italian printers of the seventeenth century even had *y* in their fonts. The omission of initial *h* in Aplosporidium is simply the omission of a silent letter which is not a letter but a diacritical mark in the original Greek. The Germans often used *ö* for *oe* even in Latin words where no transliteration is involved.

The rules are not really precise as to the language in use. Greek and Latin are mentioned, but it is not made clear that we are to transliterate Greek into Latin according to classical Latin custom rather than according to the varying usages of modern languages. *Strombidion* is a correct transliteration from Greek, but the word remains Greek in form, not Latin. Doubtless an educated Roman would have had no difficulty in understanding the Greek ending. The only course seems to be to enforce correct transliteration into Latin by amending such barbarous forms as *Flebotomus*.

CHARLES H. BLAKE

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TRANSLITERATION OF RUSSIAN WORDS

WHILE it would appear from Dr. Hoare's note in SCIENCE for December 15, 1944, that my previous note (June 16, 1944) contained a view contrary to his, the truth appears to be that we are "on the same side of the fence."

I agree thoroughly that a universally applicable Russian transliteration system is an admirable idea, and I hope that such a system will be adopted eventually.

The second part of the matter under discussion touches a somewhat different point. I believe that a person using a transliterated Russian word, be it on a file card or in a research notebook, will be compelled, at one time or another, to attempt to pronounce it in talks with the fellow workers. It is at this point that a difficulty will enter if the transliteration system used contains letters or symbols which are not found in this person's native alphabet. I simply feel that this trouble is best resolved by the use of closest phonetic counterparts in any given language for the Russian letters. Thus, for an English-speaking person the use of ordinary English letters would appear to be a good solution. This is essentially what is done in the *Chemical Abstracts* system. Incidentally, I should like to point out that the change of the Russian orthography a quarter of a century ago did not invalidate the C.A. system. In effect, the change of orthography affected only the total number of letters in the alphabet by elimination of letters which already had their phonetic counterparts (much to the delight of schoolboys, I can assure you). Thus, the phonetic features of the language were unchanged and the *Chemical Abstracts* system is perfectly usable as a pretty good phonetic transliteration system for both new and old orthographies.

The spelling of Czech in my note was my own personal oversight. Incidentally, this word presents some interesting points. It seems to me that for an English person the spelling "Chekh" is closer to the currently used pronunciation than is the usual "Czech" spelling. Frankly, I am at a loss as to how an English-speaking person would pronounce the C-z combination.

In closing this discussion, permanently I hope on my part, I wish to add that, inasmuch as Russian is my native language, it is possible that I fail to see some of the difficulties encountered by a non-Russian speaking person. I avoid the transliteration difficulties, etc., by keeping notes, etc., in whichever language is necessary.

G. M. KOSOLAPOFF

DAYTON, OHIO

SCIENTIFIC BOOKS

SCIENCE IN THE UNIVERSITY

Science in the University. By members of the Faculties of the University of California. 332 pp. 10 photographic plates. 31 figures. Berkeley and Los Angeles: University of California Press. 1944. $3.75.

THE title of this interesting volume is a misnomer. The individual reader or librarian who should order it under the impression that "Science in the University" relates broadly to either might find the book disappointing. Actually it is a compilation of occasional addresses and papers by 19 scientists[1] of the University of California, published in commemoration of the seventy-fifth anniversary of the university's founding. About half of the chapters are concerned with state and regional topics such as "The California Current," "Evolution of a Sierran Landscape" and "Subsidence and Elevation in the Los Angeles Region," or with specific contributions made by University of California scientists to genetics, hydrography,

[1] Robert Grant Aitken, J. R. Oppenheimer, Joel H. Hildebrand, Carl L. A. Schmidt, G. Ross Robertson, Jakob Bjerknes, H. U. Sverdrup, William C. Putnam, U. S. Grant, O. L. Sponsler, Richard B. Goldschmidt, Charles B. Lipman, Claude E. Zobell, Ralph W. Chaney, Loye Miller, D. R. Hoagland, J. M. D. Olmsted, Knight Dunlap and S. J. Holmes.

paleobotany, ornithology, plant nutrition and other scientific areas. A minority of the chapters in this volume summarize the character and achievements of whole fields of science.

As to the merits of the varied technical presentations, the present reviewer is not competent to judge. As to the papers which are broad in scope, he finds them excellent examples of popular exposition, notably Professor Aitken's paper on astronomy, "Driving Back the Dark"; Professor Putnam's essay on a Sierran landscape; the study of longevity of bacteria in old soil and mud bricks by the late Dean Lipman; and Professor Chaney's "Trees and History."

Citizens of California are entitled to pride in this anniversary record of scientific work at the university which they have so generously supported. Other general readers will find the volume one of rewarding value if they approach it aware of its actual contents, ready to skip judiciously, but braced also to read with care material which is significant and illuminating.

RAYMOND WALTERS

UNIVERSITY OF CINCINNATI

MEDICAL EDUCATION

Medical Education in the United States Before the Civil War. By WILLIAM FREDERICK NORWOOD. xvi + 487 pp. Philadelphia: University of Pennsylvania Press. 1944. $6.00.

IN the preparation of this history of medical education in the United States before the Civil War, the author undertook a very difficult and arduous task involving the location and study of many documents in widely separated areas.

He has made available in a single volume rather detailed factual data in regard to the medical schools established during the period covered. Much of the material included has until now been relatively inaccessible in the libraries of the country.

The first few chapters are devoted to a consideration of the status of medical practise and the initiation of medical education during the Colonial period. There follows by states and districts a description of the development of the individual medical schools and their history up to the time of the Civil War. The last few chapters deal with various general aspects of the development of the American system of medical education during the period covered.

Although a consideration of the schools on a geographic basis, following in a general way the settlement of the country, is probably the logical method of presentation, it is somewhat difficult in certain instances to follow the historic sequence. The lack of stability of most of the schools and the frequent movement of faculty members from place to place tends to be confusing to the reader. However, it should be pointed out that it is the events themselves rather than the author's presentation that is confusing.

The author is undoubtedly correct in stating that extension of the scope of the study to include developments in the field of medical education after the Civil War "would make of the book too bulky a tome." However, this volume in itself offers little opportunity for correlation with the present and stimulates the hope that it may be supplemented by a history of medical education subsequent to the events so ably recorded by Dr. Norwood.

The bibliography and the index of personal names are in themselves a valuable contribution to the literature on the history of medical education. The activities of many physicians, whose biographies have never been recorded, can be traced through the index of personal names.

In this and other respects the book will prove to be of great value for reference and will be frequently used by those interested in the period covered by it.

H. G. WEISKOTTEN

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STARCH

Chemistry and Industry of Starch, Starch Sugars and Related Compounds. By RALPH W. KERR. 472 pp. Argo, Ill.: Academic Press, Inc. 1944. $8.50.

WITH the assistance of fourteen co-authors, Dr. Kerr has prepared a very readable text dealing with the art, science and technology of the starch industry. The book is quite comprehensive in its citations to current literature, although the treatment is of necessity brief. The interweaving of scientific information with description of the art and technology has been very well done. Some of the reports of practices in the American industry can be found in no other text.

The subject-matter has been conveniently and somewhat arbitrarily distributed under five major headings: "Occurrence," "Preparation," "Properties," "Reactions" and "Uses." The section on "Properties" suffers as a result of this organization. Much of the material which could be treated in this section has been discussed under other headings; *e.g.*, viscosity and gel properties are discussed as control methods under "Preparation"; hydrogen bonding, as an interpretation of chemical characteristics under "Reactions." The section on "Properties," however, serves the worthwhile purpose of focussing attention on the separation and properties of starch fractions, particularly amylose. Considerable practical information, unavailable elsewhere in organized form, is to be found under the heading of "Uses."

In general the book is free of typographical errors. The shift in style and the repetition often characteristic of books by co-authors have been fairly well elim-

inated. An exception to this is the conflict in the views on dextrinization expressed in Chapters XII and XIII. In some of the treatment of the more controversial subject-matter, the vague speculation and non-conclusive experimental detail characteristic of older texts still persist. These deviations from the clear description of industrial operations and concise presentation of certain phases of the scientific literature detract from the general objectivity of the book.

The book is timely and a contribution to both the scientific and the technological literature.

R. M. HIXON

REPORTS

THE BOTANICAL WORK OF THE CINCHONA MISSIONS IN SOUTH AMERICA

WHEN the Japanese invaded the Dutch East Indies in the late winter of 1942, our primary source of quinine was suddenly and unexpectedly cut off. Since our stock piles of this essential drug were inadequate for the urgent needs of a long war, the critical problem was placed in the hands of the Board of Economic Warfare (now the Office of Foreign Economic Administration). Negotiations were gotten under way almost at once with the several Andean republics which a century earlier had produced the world's quinine supply. Although the cinchona agreements consisted essentially of our guarantee to buy all bark above a certain minimum alkaloid content, to furnish technical aid to the bark harvesters and dealers and to establish a plantation program, in exchange for sole buying privileges, such negotiations are traditionally slow, and it was October, 1942, before the first field party was able to leave Washington.

Dr. F. R. Fosberg and myself, the first two botanists of the cinchona program, arranged to stop off in Guatemala in order to study briefly the cinchona plantations there before starting our search for wild species in Colombia. Our first surveys were in the Santander provinces of northern Colombia, in the Eastern Andes near Pamplona, since small lots of bark from this region were already appearing on the market. In the months which followed we laboriously studied the forests of the three ranges of the Colombian Andes and came to know in a general way the basic distribution of cinchona species there. *Cinchona pubescens*, an inferior species, was found to occur throughout all three ranges, although it is relatively richer in alkaloids in the eastern range. *Cinchona officinalis*, which usually produces bark of high quality, occurs in Colombia only in the eastern range. This range was found to possess still another source of quinine, quite unsuspected when our work was originally planned. In December, 1942, it had been my good luck to run into large stands of a race of *Remijia pedunculata* on the west slopes of the Eastern Andes north of Bucaramanga. We were astonished to find that the bark of this non-cinchona gave up to 3 per cent. of quinine sulfate with very little admixture of other alkaloids. This same species extends along the eastern foothills of the Colombian Andes from Florencia to Villavicencio, near the type locality where its alkaloid production is very low. The low percentage of quinine may be due to the very sterile, sandy soil, since the high-quality variety grows on deep rich clay in Santander. Another piece of good fortune not foreseen in our original plans was the rediscovery of *Cinchona pitayensis* in the Central Andes, in Cauca province, both by Dr. Fosberg and myself. This little known species had been supposed to be a botanical rarity with very limited geographical distribution. Nevertheless, it turned out not only to be relatively abundant in southern Colombia, but also to be the species richest in alkaloids, with an average of 3 per cent. of quinine sulfate and 5 to 6 per cent. of total crystallizable alkaloids.

In July, 1943, I went to Ecuador to inaugurate exploratory work in the northern provinces, where no cinchona species had ever been collected. My first expedition had as its aim the discovery of *C. pitayensis*, unreported in Ecuador, and in August, 1943, the first-known Ecuadorian stands of this high-quality species were found on the west slope of the Western Andes in the province of Carchi. It occurs in a zone between altitudes of 8,500 and 10,000 feet, and had been overlooked previously because of the tradition in Ecuador that the best races of cinchona occur between 3,000 and 5,000 feet. Explorations were continued and during the next twelve months the belt of *C. pitayensis* was followed southward through the provinces of Carchi, Imbabura, Pichincha and León, more than a hundred miles and well into the southern hemisphere. *Cinchona officinalis*, on the other hand, which had been known previously only in Loja and Azuay provinces of southernmost Ecuador, was followed northward to the Colombian frontier. *Cinchona pubescens* was found to occur throughout the Ecuadorian Andes, and although its bark is generally low in alkaloids, especially quinine, it occasionally produces local races which are surprisingly rich. Several other species were found in the course of survey work, but are of more botanical interest than economic importance. The importance of botanical surveys was demonstrated early in our program, as can be seen from the foregoing, but it was only during the winter of 1943–1944, a full year later, that the Colombian and Ecuadorian cinchona

missions began to receive reinforcements in the form of a number of competent botanists. The survey work in the trackless and precipitous Andean forests of Ecuador just outlined would never have been possible without the help of W. H. Camp, W. B. Drew, G. W. Prescott and Ira L. Wiggins.

Although the best wild cinchona ever known, from which came the cultivated "Ledger" varieties, was discovered in Bolivia, the delicate diplomatic situation prevented the establishment of an official cinchona mission there. However, the best Bolivian barks, like those of the classic Loja region of southern Ecuador and northern Peru, had been exterminated through a destructive exploitation lasting two centuries. The highest quality bark still remaining in Bolivia and much of Peru is so inaccessible that its exploitation in any quantity is almost impossible. Consequently, the great volume of cinchona bark resulting from our work during more than two years has originated primarily in Colombia and Ecuador, even though it is of somewhat inferior quality.

The single factor of greatest importance in our work, to which I should attribute much of its success, has been our ability to obtain prompt and accurate analyses of our field samples of bark. No praise can be too high for the chemists who pioneered the cinchona mission laboratories now operating in Bogotá, Quito, Lima and La Paz. In cinchona "booms" of previous centuries, analyses could be made only after the shipment reached Europe, months after the bark had been bought and the proceeds spent. This situation led not only to fantastic speculation on good bark, but to excessive traffic in worthless barks, and one still hears reminiscences of fortunes made or lost overnight in the "quina" or "cascarilla" business. Through prompt analyses we were able to stop the harvest of poor barks and to encourage the production of good ones. Many species of Remijia, Ladenbergia and other rubiaceous genera closely resemble cinchona to the untrained eye, and our technical aid, both botanical and chemical, has saved many thousands of dollars which dealers would otherwise have "invested."

The quotas of cinchona bark set up in 1942 (which I am not free to mention) have been greatly exceeded by the supplies already harvested, and the cinchona mission botanists are returning one by one to the United States. As a consequence, we may expect publication of extraordinarily important researches on cinchona and related genera, to augment the very few publications which have already appeared on current work.[1, 2, 3] The availability of analyses has brought to light physiological distinctions between species, varieties and forms not heretofore suspected, and has provided an important new approach to the interpretation of a complex and badly misunderstood genus. From Dr. W. H. Camp and Dr. F. R. Fosberg, especially, we may certainly anticipate much enlightenment on the taxonomy of this difficult group of plants.

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[1] W. H. Hodge, *Jour. N. Y. Bot. Gard.*, 45: 32–43, 1944.

[2] F. Rosengarten, "History of the Cinchona Project of Merck and Co., Inc., and Experimental Plantations, Inc. 1934–1943." 45 pp., 82 figs. Rahway, N. J. 1944.

[3] W. C. Steere, *Flora (Revista Inst. Ecuat. Cienc. Nat.)* 4: 1–9, 1944.

SPECIAL ARTICLES

ORAL ADMINISTRATION OF PENICILLIN IN OIL

IT has been generally accepted that the various salts of penicillin in an aqueous media can not be administered orally[1, 2, 3] due to its rapid inactivation by gastric acidity. However, all the penicillin activity is not lost in the stomach. This was demonstrated by some preliminary experiments in which mice were completely protected against a thousand or more lethal doses of a virulent culture of hemolytic streptococci by mixing the dry sodium salt of penicillin in their regular diet. Approximately five to ten times the equivalent of a dosage of 100,000 units of penicillin per day in human beings was required for complete protection. This as well as data[2, 4, 5] that have appeared in the literature indicates that penicillin can be absorbed from the small intestine.

It seemed reasonable to believe that if penicillin could be protected from the gastric acidity and yet be available for absorption from the small intestines it could be administered successfully by mouth. Accordingly a series of experiments was undertaken in this regard. Enteric coated penicillin tablets appeared to be one possible method of accomplishing this; however, it was found in agreement with others[6] that consistent blood levels of penicillin in dogs could not be obtained. This was probably due to the variability in the time and the location at which the enteric coating disintegrated in the gastro-intestinal tract. Next the

[1] E. P. Abraham, H. W. Florey *et al.*, *Lancet*, 2: 177–188, 1941.

[2] C. H. Rammelkamp and C. S. Keefer, *Jour. Clin. Invest.*, XXII: 425–437, 1943.

[3] F. J. Thompson, *Jour. Am. Med. Asn.*, 126: 403–407, 1944.

[4] H. M. Powell and W. A. Jamieson, *Jour. Ind. State Med. Assoc.*, 35: 361–362, 1942.

[5] C. H. Rammelkamp and J. D. Helm, *Proc. Soc. Exp. Biol. and Med.*, 54: 324–327, 1943.

[6] Personal communication to B. W. Carey at Lederle Laboratories, Inc., Pearl River, N. Y. from C. S. Keefer, Boston, Mass.

simultaneous administration of penicillin and an antiacid was considered but discarded probably as being impracticable because of various adverse reports which have appeared in the literature.[2, 3] Finally it appeared it might be practical to utilize the fact that little if any fat-splitting takes place under the acid conditions of the stomach and that most of the digestion and breakdown of fats occurs in the small intestine. It is the purpose of this report to present our experience in the preparation and stability of and the human and animal experiments carried out with oil or fat solutions or dispersions of penicillin.[7]

Experience with various extraction and purification procedures for penicillin indicated that it was relatively stable in many organic solvents. Solutions of penicillin acid in an extensive list of naturally occurring fats and oils were prepared by the addition of an ether solution of penicillin acid to the fat or oil. The ether was removed subsequently under vacuum. It was found that the solubility of 300 to 400 units per mgm penicillin acid varied considerably between different oils and fats. One of the best in our series was cottonseed oil in which it was possible to obtain a concentration of 7,500 to 10,000 units of penicillin per ml of oil.

Stability tests on the solutions of penicillin acid in oils, with and without the addition of antioxidants such as α-tocopherol, were disappointing. The half-life of the penicillin activity at room temperature of even the best preparations was from a few days to a week. Because of the apparent instability of penicillin acid in oils or fats no animal tests appeared warranted.

Finally suspensions or dispersions of the Na, Ca, Mg and NH_3 salts of penicillin in various oils were prepared. Many of these preparations are very stable and have been kept at room temperatures for periods of two or three months without any apparent loss in activity. These results are in agreement with those reported by Romansky.[8]

For the trials on oral administration of penicillin in oil suspensions of the Na and Ca salts of penicillin, 150 to 300 units per mgm were prepared in cottonseed oil. These suspensions were dispensed in gelatin capsules made up to contain either 10,000, 25,000 or 50,000 units per capsule. Fig. 1 shows a typical example of the blood and urinary levels of penicillin over an eight-hour period obtained by the oral administration as a single dose of approximately 90,000 units of Na penicillin dispersed in cottonseed oil to an 86 kilogram man. As shown the first urine sample taken 25 minutes after administration contained about

[7] The penicillin used in this work was supplied by the Lederle Laboratories, Inc., Pearl River, N. Y.

[8] M. J. Romansky and F. E. Rittman, SCIENCE, 100: 196–198, 1944.

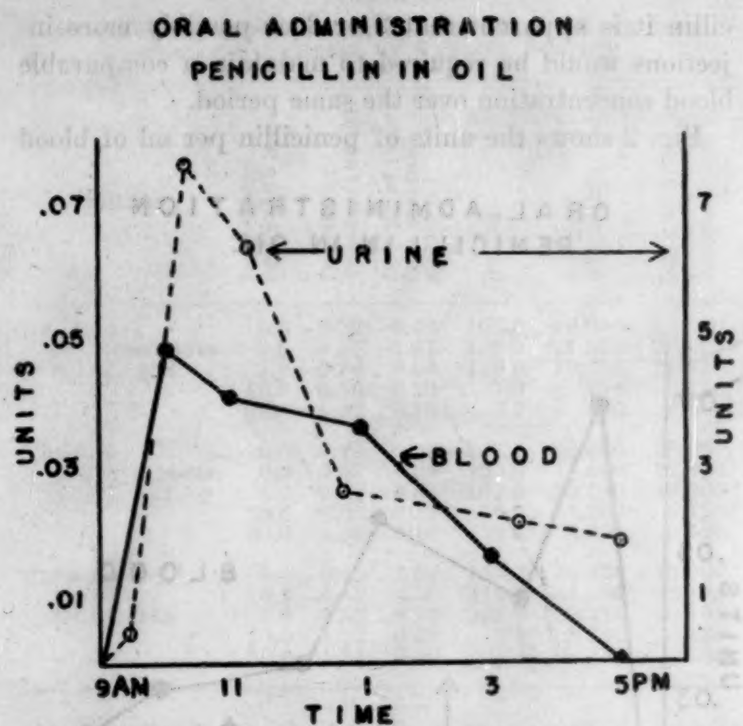

FIG. 1. Units of penicillin per ml in blood and urine after a single oral administration of 90,000 units of penicillin in oil.

0.4 units[9] of penicillin per ml. This would indicate a fairly rapid passage of the penicillin in oil through the stomach and absorption from the intestine. Maximum amounts of penicillin were found in the urine during the first two hours finally decreasing to a level of 1.8 units per ml eight hours after administration. Blood levels of approximately 0.05, 0.04, 0.04, 0.02 and zero units[10] of penicillin per ml were obtained from the 1, 2, 4, 6 and 8 hour bleedings. This as well as other trials in human beings and dogs indicate that detectable blood levels of penicillin may be maintained for considerable periods of time after the oral administration of a single dose of penicillin in oil.

It is obvious that a therapeutic blood level of penicillin will vary in different diseases and with different individuals. However, an analysis of the available data in the literature indicates that in actual clinical practice levels of between 0.03 and 0.06 units of penicillin per ml of blood are usually maintained.[2, 11] If this range is accepted as a mean value then a single oral dose of 90,000 units of penicillin in oil will maintain a fairly uniform therapeutic blood level for a period of at least four hours.

If the results presented in Fig. 1 are compared with the blood levels[2] obtained after the intramuscular injection of 20,000 units of an aqueous solution of peni-

[9] Cup plate assay: J. W. Foster and H. B. Woodruff, *Jour. Bact.*, 47: 43–58, 1944.

[10] The assay method for blood levels was developed in this laboratory and is based upon the turbidimetric measurement of the inhibition of growth of a staphylococcus culture in blood serum-broth mixtures. The results are double checked by means of plate counts. This method will be described elsewhere.

[11] W. E. Herrell, D. R. Nichols and D. H. Heilman, *Jour. Am. Med. Asn.*, 125: 1003–1011, 1944.

cillin it is apparent that 2 or 3 or possibly more injections would be required to maintain a comparable blood concentration over the same period.

Fig. 2 shows the units of penicillin per ml of blood

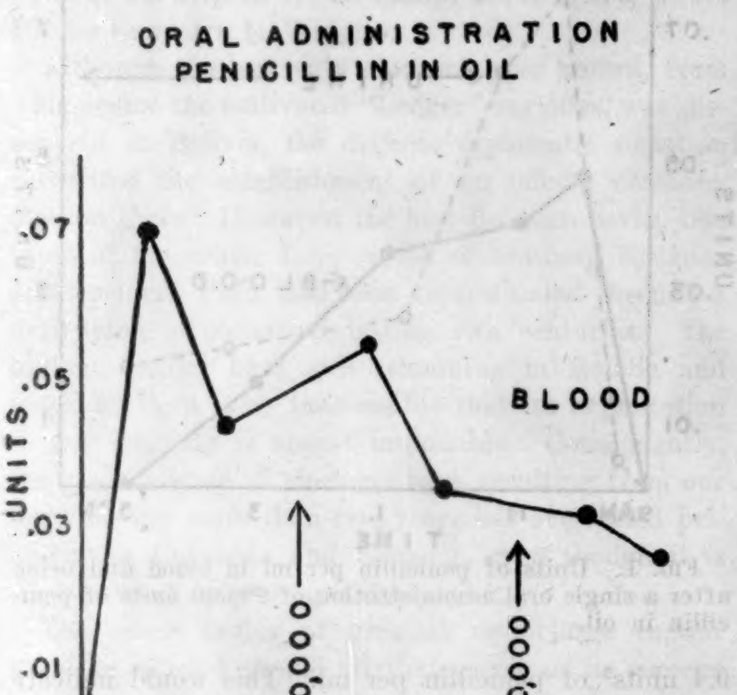

FIG. 2. Units of penicillin per ml of blood after an initial oral dose of 90,000 units and two subsequent doses of 20,000 units of penicillin in oil given three and six hours later.

obtained after the oral administration of 90,000 units of penicillin in oil and two subsequent doses of 20,000 units each given 3 and 6 hours later to a human being. As indicated, a therapeutic blood level was maintained for a period of at least seven hours and only slightly less than a therapeutic level as previously defined was found after eight hours.

It is apparent that even with the oral administration of penicillin in oil a portion is inactivated probably by the gastric acidity, so that one would expect that optimum blood levels would be obtained if the dose were given on an empty stomach. This has been found to be true in dogs and is illustrated in a human in Fig. 2. The first subsequent dose of 20,000 units was given on an empty stomach with a consequent 25 per cent. increase in the penicillin blood level. The second dose of 20,000 units was given approximately one hour after a heavy meal, no apparent increase in the penicillin blood level being observed.

It should be noted that although these preliminary results indicate that a greater amount of penicillin might be required if administered orally than if by intramuscular injection, this increased use of penicillin will probably be offset by several factors. In this regard certainly the greater ease of administration both from the point of view of the doctor as well as the patient must be considered. Furthermore, for oral use less highly refined penicillin should be entirely satisfactory, thus simplifying the present procedures for the production of suitable material.

The author would like to express his appreciation to Dr. R. Bowling Barnes, director of the Physics Division, and to the various members of the staffs of the Stamford Laboratories and of the Lederle Laboratories, Inc., for their helpful advice and assistance.

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RIBOFLAVIN PRODUCTION BY CANDIDA SPECIES

THE synthesis of relatively large quantities of riboflavin (vitamin B_2) by the yeast *Candida guilliermondia* (Northern Regional Research Laboratory strain No. 488) during cultivation in a synthetic medium has been reported by Burkholder.[1] In a second communication[2] the same investigator has described the influence on riboflavin formation of such factors as the concentration and type of carbohydrate, nitrogen and inorganic salts in the medium. For example, it was noted that whereas a riboflavin yield of 75 µg per ml was obtained in the basal medium containing 2 per cent. glucose, the use of maltose, xylose or galactose in place of glucose resulted in a marked inhibition of riboflavin synthesis, although growth was abundant. Similarly, it was found that urea, when employed at a concentration of 0.5 g per liter, enhanced riboflavin production, while in larger concentrations (2.0 to 4.0 g per liter) it was inhibitory to the synthesis of this vitamin. Yeast extract also was shown to promote growth while inhibiting riboflavin production.

These results were confirmed in our laboratory, where it was found that riboflavin formation was extremely variable, ranging from 2 to 80 µg per ml under seemingly identical conditions; whereupon a thorough study was undertaken of the influence of each of the constituents of the medium upon vitamin synthesis. From these studies it was learned that riboflavin production was greater and that higher yields were obtained more consistently, when the trace elements (B, Mn, Zn, Cu, Mo and Fe) were omitted from Burkholder's medium. When the medium was treated with 8-hydroxyquinoline in the manner described by Waring and Werkman[3] to remove Fe, Cu, Zn, Mn, etc., higher riboflavin yields resulted. However, when each of the latter elements was added to a medium extracted with 8-hydroxyquinoline, only

[1] P. R. Burkholder, *Proc. Nat. Acad. Sci.*, 29: 166, 1943.
[2] *Idem.*, *Arch. Biochem.*, 4: 217, 1944.
[3] W. S. Waring and C. H. Werkman, *Arch. Biochem.*, 1: 303, 1943.

Fe inhibited riboflavin production. The striking effect of small concentrations of iron is demonstrated in the following experiment.

The medium was prepared by dissolving in 200 ml of distilled water 0.5 g KH_2PO_4, 0.5 g $MgSO_4 \cdot 7H_2O$, 2.0 g urea, 2.0 g aspargine and 40 g glucose. About 10 mg of 8-hydroxyquinoline dissolved in 2 ml of chloroform was added to the medium, and the mixture was shaken vigorously in a separatory funnel. The funnel was allowed to stand, and the chloroform which settled out was withdrawn. Then the medium was treated similarly with chloroform alone. This alternate treatment with 8-hydroxyquinoline in chloroform and chloroform alone was repeated until the chloroform layer was colorless. This is essentially the method of Waring and Werkman, whereby they reduced the iron content of media to 0.07 to 0.3µg per 100 ml. Twenty ml portions of the extracted medium, adjusted to pH 5.0, were placed in 500 ml Erlenmeyer flasks previously cleaned with aqua regia and then 80 ml of water, triply-distilled to remove the iron, was added to each flask. One tenth of one µg of biotin was added to each flask and $FeSO_4 \cdot 7H_2O$ was added to give various concentrations up to 50 µg of iron per 100 ml of medium. After being sterilized at 126° C. for 15 minutes, the flasks were cooled and inoculated with a suspension of yeast cells which had been washed by centrifugation from triply distilled water. The weight of cells in the fermented medium and the potency of the riboflavin in the cell-free liquor were determined after an incubation period of seven days at 30° C., during which the cultures were shaken continuously. The effect of iron on growth and riboflavin formation by two strains of *Candida guilliermondia*[4] and one strain of *Candida flareri*[4] is shown in Table 1. Riboflavin was determined by spectrophotometer. Results on culture filtrates by this method were found to be in good agreement with those obtained by fluorometric and microbiological procedures.

The results show that, when the medium was practically free of iron, both cell proliferation and riboflavin synthesis were retarded. However, the iron concentration required for maximum riboflavin formation was much lower than that needed for maximum growth. When the basal medium was supplemented with iron to raise the concentration 0.5 to 1.0 µg per 100 ml, the highest riboflavin yields were obtained. At this level, the cell crop attained by *C. flareri* was approximately 40 per cent. of that formed at the highest iron level, and that attained by *C. guilliermondia* was 80 per cent. of that formed at the highest iron level. The addition of 10 µg of iron per 100

[4]These cultures were obtained from Dr. L. J. Wickerham, Northern Regional Research Laboratory, U. S. Department of Agriculture, Peoria, Ill.

TABLE 1

EFFECT OF IRON CONCENTRATION UPON GROWTH AND RIBOFLAVIN SYNTHESIS BY CANDIDA SPECIES

Culture	Iron added µg Fe/100 ml	Dry wt. of yeast g/100 ml	Dry wt of cell-free residue g/100 ml	Riboflavin synthesized µg/ml	µg/g of dry cells	µg/g of dry cell-free residue
Candida guilliermondia NRRL 488	0.0	0.27	0.45	108.0	40,000	24,000
	0.5	0.57	0.67	123.0	21,570	18,350
	1.0	0.66	0.56	120.0	18,180	21,420
	10.0	0.89	0.26	7.2	810	2,770
	50.0	0.92	0.19	3.2	350	1,670
Candida guilliermondia NRRL 324	0.0	0.21	0.37	107.0	50,950	28,700
	0.5	0.67	0.33	125.0	18,660	37,900
	1.0	0.61	0.33	157.0	25,740	47,600
	10.0	0.89	0.35	16.5	1,850	4,720
	50.0	0.82	0.19	10.6	1,290	5,580
Candida flareri NRRL 245	0.0	0.42	1.30	195.0	44,430	15,000
	0.5	0.49	0.75	216.0	44,080	28,880
	1.0	0.55	0.72	216.0	39,270	30,100
	10.0	1.12	0.28	8.9	740	3,180
	50.0	1.31	0.52	4.1	310	788

ml sharply reduced the riboflavin synthesis, while it enhanced the yeast growth. *C. flareri*, although equally sensitive to iron, synthesized considerably more vitamin than did either of the *C. guilliermondia* strains. Although riboflavin formed by *Clostridium acetobutylicum* in cereal grain mashes is stimulated by the presence of small quantities of iron[5] and inhibited by larger quantities,[6] the concentrations involved are much greater than those described here. From experiments performed in this laboratory, the optimum iron concentration for synthesis of riboflavin by *Cl. acetobutylicum* has been found to be approximately one µg per ml, which is 100 times greater than the optimum for the Candida species.

Although the above experiment was performed with a synthetic medium similar to that described by Burkholder, it has been found that various sugars and many organic and inorganic nitrogen compounds reported to inhibit vitamin synthesis[1, 2] can be employed in media for the production of riboflavin by Candida species if proper adjustment is made of the iron level of the medium.

These studies, as well as the investigation of various procedures for the regulation of iron concentration in semi-pilot plant fermentations, will be reported in detail elsewhere.

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[5] A. Saunders and L. S. McClung, *Jour. Bact.*, 46: 575, 1943.

[6] C. F. Arzberger, U. S. Patent 2,326,425, 1943.

[7] One of the laboratories of the Bureau of Agricultural and Industrial Chemistry, Agricultural Research Administration, U. S. Department of Agriculture.

FACTORS CONTROLLING BACTERIAL DISSOCIATION[1]

LIKE very many other species of bacteria, *Brucella abortus* exhibits the phenomenon of dissociation, that is, changes in colony form, culture characteristics, cell morphology, biochemical reactions, immunological reactions and virulence. The usual change is from the antigenically active Smooth (S) type to Intermediate (I) and antigenically inactive Rough (R), Brown (Br) and other types.[2]

In the course of some experiments which had been started in an attempt to study the nature of dissociation in *Brucella abortus* significant differences in the amount of dissociation after urea treatment were encountered when different strains were used.[3] It, therefore, seemed desirable to test the relative influence of possibly inherent versus environmental factors upon the degree of dissociation, with the object of systematic selection of strains with different dissociation "potentials."

Such investigations were made possible by utilizing a new technique of single cell isolation[4] which permitted studies with pure lines (clones) thus established. For the purpose of comparative studies it was necessary to establish first a standard set of conditions in which the degree of dissociation of different cultures could be compared. Thus, it was decided to express as "dissociation rate" the percentage of dissociated colonies observed on plates made from 10 days old broth cultures (beef infusion, buffered to pH 6.8) each of which had been inoculated with one Smooth colony. A comparison was then made between dissociation rates of individual colonies arising from various single cells of one stock-culture (BAI's strain Number 19-9) and dissociation rates of individual colonies all of which originated from one colony of one isolated single cell of the same culture. The data in the accompanying table show that the progeny of a single cell isolated from a heterogenous population exhibited similar dissociation rates with a relatively small range of variation (B in Table 1), while the progeny of different single cells from the same population showed varying dissociation rates (A in Table 1). Subsequently, progenies from many single cells, isolated from various strains, were tested and the similarity of dissociation rates within clones was confirmed (see Table 1). Differences between clones were found to be statistically highly significant.[5] Substrains thus established and kept on agar slants at low temperatures have so far retained their original dissociation rates for more than six months. A systematic search for clones with low dissociation rates was successful.

TABLE 1

	A	B	C	D	E	F
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
	1	44	74	28	7	0.0
	7	45	76	32	7	0.0
	24	47	78	36	7	0.0
	25	47	80	36	7	0.3
	35	50	81	41	9	3
	37	52	82	42	9	3
	37	52	87	43	9	4
	43	54	88	44	13	5
	44	54	91		14	13
	50	56	95		15	
	55	56	95			
	55	60				
	59	60				
	63	61				
	69	68				
Dissociation constant*	40% ± 5.09	54% ± 1.74	84% ± 2.23	38% ± 1.99	10% ± 0.95	3% ± 1.27
Number of colonies counted	1640	1586	1777	1220	1687	1929

A: Dissociation rates of individual colonies originating from many single cells isolated from strain 19–9.
B: Dissociation rates of individual colonies originating from one single cell isolated from strain 19–9.
C–F: Examples of dissociation rates of colonies from strains started from single cells.
* Dissociation Constant = most representative dissociation rate of a population = $\overline{p}$ of Hendricks, *Poul. Science,* 14: 365.

On the basis of results which were obtained when the effect of a number of environmental factors on these inherent dissociation rates were studied, it appears that dissociation rates should be considered as the ability of newly arising variants to establish themselves within a population. This infers that the dissociation rate, as defined here, is only an indicator for primary inherent differences, such as growth rate or

[1] The U. S. Bureau of Animal Industry is contributing to the cost of this work.
[2] B. S. Henry, *Jour. Infect. Dis.,* 52: 374, 1933.
[3] W. Braun, *Jour. Bact.,* 46: 222, 1943.
[4] K. I. Johnstone, *Jour. Path. and Bact.,* 55: 159, 1943.

[5] For example, in an analysis of variance between clones B, C, D, E and F, according to methods described by Hendricks, *Poul. Science,* 14: 365, an F value of 413.65 was obtained, which, for the appropriate degrees of freedom, corresponds to a P level of considerably less than 1 per cent.

viability of variants, and is, therefore, affected by environmental or inherent changes which affect these factors. Some experimental proof for this concept has been obtained. For example, an increase in the density of the population appears to increase the dissociation rate. In such altered environmental conditions the absolute degree of dissociation is changed, but the relative differences between two clones, such as a high dissociating one and a low dissociating one, are retained. Another example is provided by the results obtained when the effect of the pH of the environment was studied. Thus, a high dissociating strain showed 50 per cent. dissociation at pH 6.6 and 2 per cent. at pH 7.4, while a low dissociating strain showed 1 per cent. dissociation at pH 6.6 and none at pH 7.4. Tests with other strains, as well as observations with buffered and unbuffered broth, provided further proof that the pH of the environment affects the relative degree of dissociation (by affecting growth rates?), however, always within the limits of the inherent factors which determine dissociation rate; *i.e.*, environmental influences which lower the dissociation rate will decrease the dissociation rate of a high and a low dissociating strain proportionally. It should now be possible to test the relationship between dissociation rate and rate of multiplication, ability of variants to establish themselves within a population, etc., by starting broth cultures with a known number of low dissociating Smooth cells and low dissociating Rough cells and subsequent daily counts of the number of each type present.

Work with strains started from single cells has also indicated (1) that all formerly described types of variants as well as several so far undescribed types can arise among the progeny of a single cell; (2) that the type of dissociation, *i.e.*, whether primarily *S* to *R* or *S* to *Br*, etc., differs between clones, and (3) that the ability to withstand toxic effects (urea solutions of high concentration) differs between clones and can be subjected to systematic selection. These apparently inherent characteristics are now being studied and special attempts are being made to establish the feasibility of selection for immunizing power. Also, an analysis of actual chemical differences between variants has been started.

A detailed account of the work on dissociation rates will be published in the near future.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

A NEW REAGENT FOR VITAMIN A

VITAMIN A can be estimated biologically, optically or chemically. Sometimes it is desirable to make determinations in the field and for such purposes only chemical methods are suitable. Antimony trichloride is the only reagent commonly used in this way. It has, however, disadvantages not found in the reagent to be described.

This material is "Super-Filtrol,"[1] a commercial adsorbent made from the aluminum silicate mineral, Montmorillonite. It is cheap, easily handled and requires no precautions in its use except that both it and the oils to be tested be anhydrous.

When even small amounts of vitamin A in solution in petroleum ether, chloroform, benzene or carbon tetrachloride are brought into contact with the reagent it immediately becomes bright blue. All polar solvents prevent or destroy the color which can not be eluted as such from the particles of the solid. This color may be utilized as a measure of vitamin A potency in fish liver oils, and presumably for the analysis of food extracts if they are anhydrous. Materials other than fish oils have not been tested.

The color may be estimated visually by comparison with a series of standards prepared by mixing varying proportions of powdered cobalt glass with the reagent. These are standardized against oils of known potency and may be conveniently prepared in steps of about 15,000 I.U. The color may be measured more accurately by a reflection type photoelectric colorimeter which may be made very simply by utilizing a diverging and variable slit between the photocell and the colored surface.

The color may best be developed by placing about 10 gm of "Super-Filtrol" in a small flask and covering it with petroleum ether. To this add a known weight or volume of vitamin A-containing material either as solution or just as oil, and shake. The blue color forms at once. For field use it has been found convenient to deliver the oil from the blunt end of a 20-gauge, hypodermic needle on a 1 cc syringe. Drops of the order of 7 mg are delivered with an accuracy of about 3 per cent. Two such drops, containing as little as 28 I.U. (2,000 I.U. per gm) will make a color on 10 gm of the reagent.

Shantz, Cawley and Embree[2] have shown that in the case of the antimony trichloride reaction a dehydration takes place, splitting off the terminal alcohol group and forming "anhydro vitamin A." This reaction is probably common to most or all of the materials

[1] Manufactured by the Filtrol Corporation, Vernon, Calif.

[2] E. M. Shantz, J. R. Cawley and N. D. Embree, *Jour. Am. Chem. Soc.*, 65: 901, 1943.

making a color with the vitamin, since they are all dehydrating agents of various degrees. In the case of this reagent the dehydration takes place on the surface of the particles because they have an adsorbed layer of sulfuric acid as a result of the leaching process used in their manufacture. Attempts at elution with solvents such as acetone destroy the blue color but yield an orange oil which has a different absorption spectrum from the vitamin.

Complex formation between the reagent (adsorbed sulfuric acid) and the anhydro compound appears to be the most probable explanation of the phenomenon. That such a complex is possible is indicated by the fact that carotene also makes a color even though it is a hydrocarbon.

Interesting possibilities are suggested by the principle of this assay. Not infrequently it is desirable, either for synthetic or analytic purposes, to react two substances which can not be brought into solution in a common solvent. Carrying one substance into contact with another by means of an inert adsorbent might solve such problems.

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A MODIFIED PETRI DISH FOR CONTINUOUS TEMPERATURE OBSERVATION

IN investigations upon free-living and parasitic Protozoa, I have found that greater accuracy could be obtained in using a modified Pyrex Petri dish for

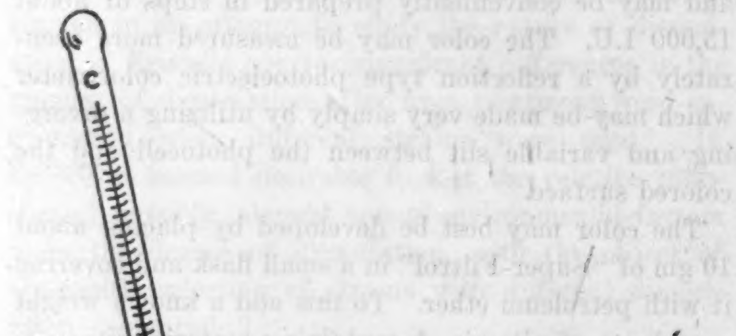

FIG. 1.

controlling and maintaining constant temperatures of culture fluids, stains and various fixing reagents.

The dish (Fig. 1) is a regulation stock four-inch Pyrex Petri dish (bottom and lid) in which a hole measuring approximately one centimeter in diameter is made close to the rim of the lid. A piece of glass tubing slightly larger than the diameter of the hole is then fused over it so that a collar is formed about one centimeter high. A piece of rubber tubing about six millimeters in length is inserted in the glass collar. Then a small clinical-type thermometer is inserted into the rubber-cushioned collar so that the bulb of the thermometer which should be immersed in the fluid comes to rest slightly above the bottom of the dish. Direct temperature readings may now be made of the contents of the dish without removing the lid.

Many investigators in protozoology and parasitology make smeared preparations directly on cover-slips which are then dropped into the heated fixing fluid. With this dish, it is possible to kill and fix the organisms at a definite temperature and still be able to maintain the correct temperature over a given period of time with the lid covered. In staining, it has proved to be extremely useful, especially in the Feulgen test for thymonucleic acid where a given temperature must be maintained for a definite period of time.

The dish should prove to be convenient and useful for protozoologists, parasitologists and those working with small animals where greater accuracy is desired in this phase of technical work. Entomologists may find the dish useful by simply using it as a cover with a stoppered opening through which fluids may be added to developing embryos without removing the lid thereby lessening the possibility of contamination.

It seems likely that with greater emphasis being placed upon research in parasitology and tropical medicine, there will be considerable usage for a dish of this nature.

The author wishes to express his thanks to Dr. James A. Harrison for his aid in construction of the dish.

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